TRANSACTIONS OF THE

INSTITUTION OF NAVAL ARCHITECTS.

1879.

UNIVERSITY OF ILLINOIS
LIBRARY

Class 623.06 Book SOC Volume 20

REMOTE STORAGE
My 08-15M

The person charging this material is responsible for its return to the library from which it was withdrawn on or before the **Latest Date** stamped below.

Theft, mutilation, and underlining of books are reasons for disciplinary action and may result in dismissal from the University.

To renew call Telephone Center, 333-8400

UNIVERSITY OF ILLINOIS LIBRARY AT URBANA-CHAMPAIGN

L161—O·1096

TRANSACTIONS

OF THE

INSTITUTION OF NAVAL ARCHITECTS.

VOLUME XX.

EDITED BY GEORGE HOLMES,

SECRETARY OF THE INSTITUTION.

1879.

OFFICE: 5, ADELPHI TERRACE, LONDON, W.C.

SOLD BY HENRY SOTHERAN & CO., 36, PICCADILLY, LONDON, W.;

J. BAER & CO., PARIS AND FRANKFORT-ON-THE-MAIN.

[*All rights reserved.*]

UNWIN BROTHERS,
PRINTERS.

CONTENTS.

PAGE

List of Officers v

List of Honorary Members, Honorary Associates, Members, and Associates vii

Objects of the Institution xiii

Bye-Laws and Regulations xiii

Introductory Proceedings xix

PAPERS AND DISCUSSIONS.

On Armour-Plating Ships of War. By Admiral Sir R. Spencer Robinson, K.C.B., F.R.S.; Vice-President 3

On Armour for Ships. By N. Barnaby, Esq., C.B., Director of Naval Construction of the Royal Navy; Vice-President 27

Discussion on the two preceding Papers 33

On Naval Guns. By C. W. Merrifield, Esq., F.R.S.; Associate Member of Council 46

Discussion on the above Paper 50

On the True Nature of the Wave of Translation, and the Part it Plays in Removing the Water out of the Way of a Ship with Least Resistance. By J. Scott Russell, Esq., F.R.S.; Vice-President 59

Discussion on the above Paper 84

*On a Few Years' Experience of the Screw Propeller: Its Lessons and Results. By Arthur J. Maginnis, Esq. 88

Discussion on the above Paper 100

On the Resistance Given to Screw Ships by the Action of the Screw Propeller, and how to Remedy it. By Robert Griffiths, Esq. 104

Discussion on the above Paper 108

Introductory Proceedings, Friday Morning 116

On the Steam Trials of H.M.S. *Iris*. By J. Wright, Esq., Engineer-in-Chief, Admiralty; Vice-President 117

Discussion on the above Paper 127

On the Structural Arrangements and Proportions of H.M.S *Iris*. By W. H. White, Esq., Assistant-Constructor of the Navy; Member of Council... 133

Discussion on the above Paper 142

On the Minimum Area of Blade in a Screw Propeller Necessary to Form a Complete Column. By J. H. Cotterill, Esq., M.A., F.R.S., Professor of Applied Mechanics at the Royal Naval College; Associate 152

Discussion on the above Paper 160

On Twin Ship Propulsion. By G. C. Mackrow, Esq., Member 162

Discussion on the above Paper 169

* This Paper was awarded the Gold Medal offered by Mr. R. Carlyle, for the best Paper on Screw Propulsion. Read at the Spring Meeting.

PAGE

On Some of the Causes of Failure of Crank-Shafts in Marine Engines. By J. T. Milton, Esq., Surveyor to Lloyd's Registry of British and Foreign Shipping ; Member 180
Discussion on the above Paper 191

On the Legal Load Line, and some Remarks Relating to the Principles of Safe Freeboard. By N. E. Porter, Esq., Associate 196
Discussion on the above Paper 203

On the Double Power Floating Dock and the Hydraulic Grid. By Latimer Clark, Esq., C.E.... ... 204
Discussion on the above Paper 209

Introductory Preceedings, Saturday Morning 211

On the Most Powerful Ironclad. By Admiral J. H. Selwyn, R.N. 211
Discussion on the above Paper 224

On a System of Mechanical Shipbuilding. By J. Humphrys, Esq., Member 224
Discussion on the above Paper 235

On Lloyd's Yacht Registry. By Dixon Kemp, Esq., Associate 245

On Sir William Thomson's Navigational Sounding Machine. By P. M. Swan, Esq., Member 253
Discussion on the above Paper 258

On the Oscillometer: An Instrument for Measuring the Angle through which a Ship Rolls or Pitches at Sea. By R. Clarke, Esq. 259

Concluding Proceedings 261

Memoir of the late John Penn, F.R.S. 263

Memoir of the late William Froude, M.A., F.R.S. 264

Nominal Index 273

OFFICERS OF THE INSTITUTION.

President.

THE RIGHT HON. LORD HAMPTON, G.C.B., D.C.L., &c.

Vice-Presidents.

H.R.H. the Duke of Edinburgh, K.G., Rear-Admiral.
The Right Hon. Hugh C. E. Childers, M.P.
The Right Hon. G. J. Göschen, M.P.
The Right Hon. Lord John Hay, C.B., Rear-Admiral.
The Right Hon. Sir John Dalrymple Hay, Bart., M.P., C.B., D.C.L., F.R.S., Admiral.
The Right Hon. W. H. Smith, M.P.
The Hon. Arthur Duncombe, Admiral.
Sir Alexander Milne, Bart., G.C.B., Admiral.
Sir Frederick W. E. Nicolson, Bart., C.B., Vice-Admiral.
Sir Cooper Key, K.C.B., F.R.S., F.R.G.S., Vice-Admiral.
Sir Robert Spencer Robinson, K.C.B., F.R.S., Admiral.
Sir W. Houston Stewart, K.C.B., Rear-Admiral.
Richard Abethell, Esq.
Nathaniel Barnaby, Esq., C.B.
Thomas Chapman, Esq., F.R.S., F.S.A., &c.
Robert Hall, Esq., C.B., Rear-Admiral.
E. J. Reed, Esq., C.B., F.R.S., M.P.
John Scott Russell, Esq., F.R.S.
J. D'Aguilar Samuda, Esq., M.P.
James Wright, Esq.
Joseph Woolley, Esq., M.A., LL.D.

Members of Council.

F. K. Barnes, Esq.
Henry Cradock, Esq.
J. B. C. Crossland, Esq.
Peter Denny, Esq.
William Denny, Esq.
Robert Duncan, Esq.
John Dudgeon, Esq.
Joshua Field, Esq.
Henry Green, Esq.
E. J. Harland, Esq.
William John, Esq.
Henry H. Laird, Esq.
A. D. Lewis, Esq.
Benjamin Martell, Esq.
Francis Martin, Esq.
James Martin, Esq.
William Moody, Esq.
Henry Morgan, Esq.
James R. Napier, Esq., F.R.S.
John Napier, Esq.
William Owen, Esq.
John R. Ravenhill, Esq.
George Banks Rennie, Esq.
William B. Robinson, Esq.
Alfred B. Sturdee, Esq.
Philip Thornton, Esq.
Bernard Waymouth, Esq.
John White, Esq.
W. H. White, Esq.
Charles H. Wigram, Esq.

Associate Members of Council.

Sir William G. Armstrong, Kt., C.B., D.C.L., F.R.S.
Frederick J. Bramwell, Esq., F.R.S.
Thomas Brassey, Esq., M.P.
Charles W. Merrifield, Esq., F.R.S.
Sir James Ramsden, Kt.
Rear-Admiral G. Randolph, C.B.
T. E. Symonds, Esq., Captain R.N.

Treasurer.

J. D'Aguilar Samuda, Esq., M.P. (Vice-President).

Auditors.

John Corry, Esq. (Associate).
James Mackenzie, Esq. (Associate).

Secretary.

George Holmes, Esq.

LIST OF HONORARY MEMBERS, HONORARY ASSOCIATES, MEMBERS, AND ASSOCIATES.

Honorary Members.

Andresen, Jakob Boudegaard, Director of Naval Construction, Trieste.
De Lôme, Dupuy, ex-Inspector-General of Naval Construction, Paris.
Mattei, Signor F., Inspector of Naval Construction, Ministry of Marine, Rome.

Honorary Associates.

Airy, Sir George B., K.C.B., M.A., LL.D , D.C.L., F.R.S., Astronomer-Royal.
Audenet, Camille, Sub-Director of Naval Construction, Paris.
Constantine, H.I.H. the Grand Duke.
Palmella, His Grace the Duke of.
Thomson, Sir William, Kt., F.R.S.

Members.

Abethell, Richard (Vice-President).
Adams, Thomas.
Allfrey, Edward R.
Andrews, Robert Henry.
Archer, Colin.
Ardaseer, Rustomjee.
Armstrong, Robert.
Artsayooloff, Lieut. Constantine.
Atkinson, Edwin John.
Austin, Francis B.
Austin, Samuel P.

Baillie, Jonathan Robert.
Bakewell, Herbert James.
Barker, Henry.
Barnaby, Nathaniel, C.B. (Vice-President).
Barnes, Frederick K. (Mem. Council).
Barrett, Robert D.
Bartram, R.A.
Baskcomb, William B.
Bath, William.
Baxter, Samuel.
Bedbrook, James Albert.
Bell, William.
Berrier-Fontaine, Marc.
Bertin, Emile.
Bevis, R. R.
Biles, John H.
Blom, Hans A.
Bone, William John.
Boolds, H. J.
Bower, Anthony.
Broad, John Freeman.
Brock, Walter.
Brodrick, Herbert Stead.
Brown, Henry Edward.
Brown, J.
Brown, R. B.
Burmeister, Carl Christian.
Bushell, C. A.
Butler, R. J.
Byrne, St. Clare John.

Caird, James T.
Carter, Alfred.
Chilcott, William W.
Clark, George.
Claughton, Henry.
Cole, G. J.
Cole, Henry A. B.
Cooper. G. P.
Cooper, William.
Cornish, Harry John.
Costell, James.
Couchmann, Edwin R.
Cox, Elias.
Cradock, Henry (Mem. Council).
Crossland, James B. C. (Mem. Council).

Davidson, Charles.
Davidson, W.
Davis, F. B.
Dawkins, John.
Day, Charles Arthur.
Deadman, Henry Edward.
De Bussy, Louis.

De Carvalho, Trajano Augusto.
Delaitre, M. Pierre.
Denny, Peter, F.R.S.E., M.I.C.E. (Mem. Council).
Denny, W., M.I.C.E. (Mem. Council).
Denton, William.
De Rusett, Edwin W., C.E.
Dhunjabhoy, Jamsetjee.
Dixon, Raylton.
Dobson, William.
Dodd, Thomas James.
Doxford, R. P.
Doxford, William Theodore.
Duck, George N.
Dudgeon, John (Mem. Council).
Duncan, Robert.
Dunn, James.

Edwards, H. S.
Edwards, N. P.
Edwards, Thomas.
Eldridge, George.
Elgar, Francis.
Eltringham, J. T.
Evans, John.

Field, Joshua (Mem. Council).
Field, Telford.
Fitze, William James.
Flannery, J. Fortescue.
Forbes, William John.
Foster, Robert.
Froyne, John C.
Frykholm, J. L.

Goulaeff, E. E., Capt., F.R.S.N.A., Russian Imperial Navy.
Graham, Edwin.
Gray, John MacFarlane.
Gray, William.
Grayson, H. Holdrege.
Green, Henry (Mem. Council).
Grüson, Herr Hermann.

Hamilton, John.
Hamilton, James, junr.
Harland, Edward James (Mem. Council).
Harrington, T.
Hartness, John.
Harvey, John.
Harvey, John.
Haswell, Charles H.
Haswell, G.
Hebson, Douglas.
Henderson, John, junr.
Herreshoff, John B.
Herreshoff, Nathaniel G.
Hichborn, Philip, Naval Constructor, U. S. Navy.
Hollingum, G.
Holmes, Samuel.
Hounsom, William.
Humphrys, James.
Hunter, G. B.

Inglis, J.
Inman, Edwin.
Irvine, Robert.
Isaac, Percy L.

Jack, Alexander.
Jago, Richard.
Jamieson, J. L. K.
Jansen, H. G.
John, William (Mem. Council).
Jordan, Charles H.

Kafer, John C., Passed Assistant-Engineer, United States Navy.
Kennedy, Alex. B. W.
Kettle, T. W.
Key, John.
Keydell, E. A.
Kinghorn, John.
Kirk, A. C.
Kirkwood, James.

Laird, Henry Hyndman (Mem. Council).
Lang, William C.
Langtry, C.
Lapthorn, Samuel.
Laslett, William Henry.
Lewis, Alfred David (Mem. Council).
Littlejohn, W. G.
Lutschaunig, Victor.

Mackrow, George C.
Maier, Fritz F.
Martell, Benjamin (Mem. Council).
Martin, Francis (Mem. Council).
Martin, James (Mem. Council).
McDougall, Niel.
McKenzie, J.
McLaine, Alexander.
McMillan, J., junr.
Menzies, Thomas.
Milton, J. T.
Mintonye, W. L., Naval Constructor, U.S. Navy.
Mitchell, Charles.
Moody, William (Mem. Council).
Morgan Henry (Mem. Council).
Morley, Thomas.
Mumford, W. T.
Munday, Charles Frederick.

Napier, James R., F.R.S. (Mem. Council).
Napier, John (Mem. Council).
Napier, R. A., Lieut. R.N.R.
Normand, J. A.

Oswald, Thomas Ridley.
Owen, William (Mem. Council).

Parker, William.
Paulsen, William.
Pearce, William.
Penn, John.
Penney, George F.
Phillips, Thomas.
Pirrie, W. J.
Pook, Samuel H., Naval Constructor, U.S. Navy.
Potter, W. H.
Pratten, W. J.

44

Price, J.
Pryer, George.
Purdie, James.

Rait, Henry Milnes.
Ramage, Richard.
Rankine, John.
Ravenhill, John R. (Mem. Council).
Reed, E.J., C.B., F.R.S., M.P. (Vice-President).
Reed, R. J.
Reid, John.
Rendel, George W.
Rennie, George Banks (Mem. Council).
Rennie, William.
Richards, James.
Richardson, J., Wigram.
Rickard, George James.
Ritchie, Joseph H.
Robertson, Duncan.
Robinson, William B. (Mem. Council).
Romako, J. E. Von.
Rubie, George P.
Russell, John Scott, F.R.S. (Vice-President).

Samson, P.
Samuda, J. D'Aguilar, M.P. (Vice-President and Treasurer).
Samuelson, Martin.
Schlesinger, C. A.
Scott, James Edward.
Scott, John.
Scott, Michael.
Seaton, Albert Edward.
Seccombe, William Edward.
Seddon, J. A.
Sells, Charles.
Sennett, Richard Engineer, R.N.
Shepherd, John William.
Short, J. Y.
Shilston, Thomas,
Simons, William.
Simpson, James.
Sinnette, James Lewis.
Sivewright, George W.
Skinner, James.
Smith, Wasteneys, C.E.
Spence, Henry Grant.
Stanbury, George.
Steele, Robert.
Stephen, A.
Stephenson, George Robert.
Stockwell, John Alfred.
Stoney, Bindon, Blood.
Storey, W. C.
Sturdee, Alfred B. (Mem. Council).
Summers, Thomas.
Swan, Henry Frederick.
Swan, Peter Morrison.

Thearle, Samuel James Pope.
Thompson, Cornelius.
Thompson, J. E.
Thompson, Robert.
Thomson, J. Rodger.
Thomson, G. Paul.
Thornton, Philip (Mem. Council).
Tideman, Bruno Joannes.
Tobin, J. A. (Engineer Corps, U.S. Navy).
Trew, T. Vernon.
Trevithick, J. G.
Trickett, John.
Truscott, James H.
Turnbull, Thomas, junr.
Turpin, James.

Vine, Samuel John.

Wain, William.
Walker, William.
Walpole, Thomas.
Ward, John.
Warren, E. C.
Watson, William.
Watts, Luther.
Watts, Philip.
Waymouth, Bernard (Mem. Council).
Welch, John A.
West, Henry Hartley.
White John (Mem. Council).
White, John S.
White, R. Saxton.
White, William Henry.
Whyte, William.
Wigram, Charles H. (Mem. Council).
Wigram, Clifford.
Wildish, James George.
Williams, John Mann.
Williamson, James.
Williamson, Thomas.
Wilson, Alexander Hall.
Wilson, Theodore D., Naval Constructor, United States Navy.
Wilson, J. P.
Wimshurst, Henry.
Wimshurst, James.
Withy, Edward.
Wright, James (Vice-President).

Young, Sidney.

Associates.

Abegg, George L.
Abernethy, James.
Adcock, Francis L.
Ailsa, Marquis of.
Alzola, Captain, Royal Spanish Naval Commission.
Andersen, Edward Johan.
Antoine, Charles.
Armstrong, Robert.
Armstrong, Sir William G., Kt., C.B., D.C.L., F.R.S. (Assoc. Mem. Council).

Austin, Charles E.
Aynsley, C. Murray, C.B., Captain R.N.

Bailey, Charles Pink.
Baillie, Robert.
Barnard, Edward B.
Barr, William.
Barrow, Edward.
Bateman, John F., F.R.S., F.G.S.
Baxter, Wynne E.
Bell, William.
Benham, Henry.
Bethune, Charles, R. N., C.B., Admiral.
Bingham, J. R. W.
Black, Thomas.
Blackmore, Edward.
Blanco, Don Antonio, Capt. Spanish Royal Navy.
Bone, James.
Bragge, William.
Braithwaite, Isaac.
Bramwell, Frederick J., F.R.S. (Assoc. Mem. Council).
Brassey, Thomas, M.P. (Assoc. Mem. Council).
Brereton, Robert P.
Bridson, Harry.
Broomham, William.
Brown, A. H.
Brown, David.
Bruce, Gainsford.
Brum, Crescencio.
Brunlees, James.
Buccleuch and Queensberry, Duke of, K.G., D.C.L., F.R.S., F.L.S.
Burt, George.

Campbell, James.
Canter, William James, R.N.
Carmichael, Peter.
Carpenter, Hon. Walter C., Capt. R.N.
Carranza, Senor Don José.
Carttar, Edward Arundel.
Casella, Louis P.
Caveda, His Excellency H. Nava y General, Spanish Royal Navy.
Chapman, Charles, Capt.
Chapman, Thomas, F.R.S., F.S.A. (Vice-President).
Childers, Right Hon. Hugh C. E., M.P. (Vice-President).
Churchill, Richard.
Cobb, George H.
Cohn, A.
Collin, D.
Cook, A. B., Capt. L.L.M.
Cooper, J. H., Lieut.-Colonel 7th Fusiliers.
Corry, John (Auditor).
Cotterill, J. H., M.A.
Cowper, Edward A.
Crease, John Frederick, Major, R.M.A.
Cresswell, Francis Joseph.
Crickmer, Richard J.
Crofton, Edward.
Custance, Lieut. Reginald, R.N.

Da Costa, S. J.
Darling, John.
De Cuverville, Captaine Ide Cavelier.
De Haro, J.F., Capt. Spanish Royal Navy.
De Hoffman, Chevalier.
De Horsey, Algernon F. R., Rear-Admiral, R.N.
Du Buit, Charles Paul.
Dudgeon, Alexander John.
Dudgeon, William Leigh.
Duncan, George.
Duncombe, Hon. Arthur, Admiral (Vice-President).
Dunlop, Hugh, C.B., Vice-Admiral.

Eckart, William R., C.E.
Edinburgh, H.R.H. the Duke of, K.G., Rear-Admiral (Vice-President).
Edwards, F.
Egerton, Hon. Francis, M.P., Rear-Admiral.
Eidlitz, Leopold.
Elgar, Alfred.
Elphinstone, Lord.
Evans, R. D., Lieut.-Commander, United States Navy.

Fanshawe, Edward Gennys, C.B., Vice-Admiral.
Fasella, Cavaliere Felice, Italian Royal Navy.
Fenning, William M.
Fleming, William.
Footitt, Robert.
Fowler, John, F.R.S.

Garoufalias, Andrew, Lieut. Greek Royal Navy.
Goldsmith, George, C.B., Vice-Admiral.
Göschen, Right Hon. G. J., M.P. (Vice-President).
Gourlay, Henry.
Grierson, Frank Willisford.
Gumpel, Charles Godfrey.
Guppy, Thomas R.

Hall, Robert, C.B., Rear-Admiral (Vice-President).
Hall, Captain H., R.N.
Hampton, The Right Hon. Lord, G.C.B., D.C.L. (President).
Harfield, William Horatio.
Harper, G. S.
Hartmann, Wilhelm.
Harvey, John, Capt. R.N.
Hay, The Right Hon. Lord John, C.B., Rear-Admiral (Vice-President).
Hay, The Right Hon. Sir John Dalrymple, Bart., M.P., C.B., D.C.L., F.R.S., Vice-Admiral (Vice-President).
Hazi, Solozoro.

Healey, Edward C.
Hearson, Thomas Alfred, Assistant-Engineer, R.N.
Heyl, George F.
Hide, Thomas C.
Hill, E. G.
Hill, E. S., Lieut.-Colonel.
Hill, Laurence, C.E.
Hodgson, Thomas.
Hope, Sir James, G.C.B., Admiral.
Houldsworth, William.
Howell, J. Bennett.
Hughes, John.
Hyde, John Moore.

Ihlen, Nils, Lieut. Norwegian Royal Navy.
Inglis, Anthony.
Inman, William.
Isaac, Frederick.
Isaacs, Michael H.
Ivanoff, N., Capt. Russian Imperial Navy.

Jackson, Thomas T.
Jones, Sir Lewis T., K.C.B., Admiral.

Kemp, Dixon.
Key, Sir Cooper, K.C.B., F.R.S., F.R.G.S., Rear-Admiral (Assoc. Mem. Council.)
King, William Poole, F.R.S.
Knudson, Gunnar.
Korshicoff, Nicholas, Colonel, Russian Imperial Navy.
Koznakoff, Nicholas, Capt Russian Imperial Navy.

Labrano, Captain Fredrico, Imperial Italian Navy.
Lagane, Amable.
Lautrup, Christian.
Lee, John D.
Lees, H. R., Lieut. R.N.
Liardet, John Evelyn.
Liggins, Henry.
Littleboy, C. W.
Littlejohn, John.
Ljungstedt, A. G. A., Colonel Swedish Royal Navy.
Lutken, Otto, Capt. Danish Royal Navy.
Lüning, J. G.

MacIver, David, M.P.
Mackenzie, James.
Mackie, S. J.
Mackrow, Clement.
Maksimoff, Lieut. B. B., Russian Imperial Navy.
Martin, Sir Wm. F., Bart. K.C.B. Admiral.
McGregor, J.
McHardy, Wallace Bruce, Commander R.N.
Merrifield, C. W., F.R.S. (Assoc. Mem. Council).
Milne, Sir Alexander, G.C.B., Bart., Admiral (Vice-President).
Milward, Thomas.
Moffat, John.
Moore, William W.
Moreaux, C. F. da S.
Moreton, Capt. A. H. M.
Moy, Thomas.
Murray, Robert.

Nash, Charles.
Neilson, Walter M.
Newman, A. R.
Nicholson, Benjamin.
Nicholson, John.
Nicholson, Sir Frederick W. E., Bart., C.B., Vice-Admiral (Vice President).
Noel, G. H., Com., R.N.
Normanton, Earl of.
Norwood, Charles M., M.P.

Orlando, Paolo.
Page, W. Samuel.
Pearse, Mountjoy.
Perkins, James.
Petke, Fritz.
Poretchkin, Theodore.
Porter, Nicholas Ellis.
Pratt, George.
Price, Arthur, Paymaster, R.N.
Purves, John.
Purvis, Frank P.

Ramsden, Sir James, Kt. (Assoc. Mem. Council).
Randolph, George G., C.B., Rear-Admiral.
Rawson, Robert, Hon. Mem. Manchester Literary and Philosophical Society.
Rennie, John K., M.A.
Rice, J. M., United States Navy.
Robinson, M. H.
Robinson, Sir R. Spencer, K.C.B., F.R.S., Admiral (Vice-President).
Rothe, H. P., Commodore, Danish Royal Navy.
Rowan, David.
Rowley, Charles J., Capt. R.N.
Rundell, W. W.
Rundle, Cubitt S., C.E.
Russell, Norman S.

Samuelson, Bernhard, M.P.
Sandham, H.
Sasoa, Sachin.
Satrustegui, Don Patricio de.
Sandstrom, Theodore C. A., Lieut. Royal Swedish Navy.
Saunders, James Ebenezer, C.C., F.S.A., F.G.S.
Schmeman, Alexr.
Schüler, F.
Schunk, Theodore.
Scott, Lord Charles T., Capt. R.N.
Shadwell, Charles F. A., C.B., Vice-Admiral.

Siemens, Charles W., F.R.S., D.C.L.
Sinnette, George Mathias.
Small, Thomas.
Smith, Charles William.
Smith, G. Fereday.
Smith, John.
Smith, Josiah T.
Smith, The Right Hon. W. H., M.P. Vice-President.
Smyth-Pigott, J. H. W.
Spence, James Carmichael.
Spyer, Arthur.
Stewart, John.
Stewart, William Houston, C.B., Admiral (Vice-President).
Stothert, G. K.
Sudeley, The Right Hon. Lord.
Suffield, Thomas.
Swinburne, Thomas A., Captain R.N.
Symington, William, Capt. R.N.
Symonds, Thomas E., Capt. R.N. (Assoc. Mem. Council.)

Talbot, Edward.
Talbot, Edward James.
Talbot, J. William.
Tatham, Hollings.
Testa, Capt. Charles, Royal Portuguese Navy.
Thompson, Edmund.
Thomson, Andrew.
Thornycroft, John Isaac, C.E.
Thurston, Robt. H., C.E.
Tindall, William H.
Tough, R. T.
Tryon, George, C.B., Capt. R.N.

Varvelli, G. Marino.

Waddilove, Charles L., Capt. R.N.
Waltjen, Charles.
Warren, Alfred.
Watson, William.
Westhorp, Theophilus.
Wheatley, William Coe.
Wheeler, Joseph, junr.
Whyham, Maurice.
Williams, George, Rear-Admiral.
Williams, Samuel.
Williamson, R. H.
Wise, William Lloyd.
Wood, George.
Wood, George Edward.
Woolfe, J. S.
Woodall, J. W.
Woolley, Joseph, M.A., LL.D. (Vice-President.)

Yeo, John, Engineer R.N.

Zeysing, Theodore, Prussian Royal Navy.

OBJECTS OF THE INSTITUTION.

The objects of the Institution of Naval Architects—which was established to promote the Improvement of Ships, and of all that specially appertains to them—are comprised under three heads:—

First, the bringing together of those results of experience which so many shipbuilders, marine engineers, naval officers, yachtsmen, and others acquire, independently of each other, in various parts of the country, and which, though almost valueless when unconnected, doubtless tend much to improve our navies when brought together in the printed Transactions of an Institution.

Secondly, the carrying out, by the collective agency of the Institution, of such experimental and other inquiries as may be deemed essential to the promotion of the science and art of shipbuilding, but are of too great magnitude for private persons to undertake individually.

Thirdly, the examination of new inventions, and the investigation of those professional questions which often arise, and were left undecided before the establishment of this Institution, because no public body to which professional reference could be made then existed.

BYE-LAWS AND REGULATIONS.

CONSTITUTION.

1. The Institution of Naval Architects shall consist of four classes, viz., Members, Associates, Honorary Members, and Honorary Associates.

2. *Members.*—The Class of Members shall consist exclusively of Naval Architects and Marine Engineers conversant with Naval Architecture.

3. *Associates.*—The Class of Associates shall consist of persons who are qualified, either by profession or occupation, or by scientific or other attainments, to discuss with Naval Architects the qualities of a ship, or the construction, manufacture, or arrangement of some part or parts of a ship or her equipment.

4. *Honorary Members.*—The Class of Honorary Members shall consist of persons who are eligible as Members, and upon whom the Council may see fit to confer an honorary distinction.

5. *Honorary Associates.*—The Class of Honorary Associates shall consist of persons who have contributed to the improvement of ships or their equipment, and upon whom the Council may see fit to confer an honorary distinction.

ELECTION AND DUTIES OF OFFICERS.

6. The Officers of the Institution shall consist of a President, Vice-Presidents, Members of Council, Associate Members of Council (not exceeding in number one-third the number of Members of Council), a Treasurer, two Auditors of Accounts, and a Secretary or Secretaries.

7. A General Meeting of the Members and Associates of the Institution shall be held annually before Easter in each year; and at this Annual General Meeting the Members of Council, Associate Members of Council, Treasurer, and Auditors for the ensuing year shall be elected.

8. At the Annual General Meeting Members only shall vote in the Election of Members of Council, and both Associates and Members in the Election of Associate Members of Council, the Treasurer and the Auditors.

9. As soon as may be convenient after the Annual General Meeting, the newly-elected Members and Associate Members of Council shall meet and elect the President and Vice-Presidents for the year, Members and Associate Members of Council both voting.

10. *President.*—Both Members and Associates of the Institution shall be eligible for election as President. The President shall preside over all Meetings of the Institution, and of Officers of the Institution, at which he is present, and shall regulate and keep order in the proceedings.

11. *Vice-Presidents.*—Both Members and Associates of the Institution shall be eligible for election as Vice-Presidents. In the absence of the President, one of the Vice-Presidents shall preside at the General Meetings of the Institution, and shall regulate and keep order in the proceedings.

12. In case of the absence of the President and of all the Vice-Presidents, the Meeting may elect any Member of Council or Associate Member of Council, and in case of their absence any Member present to preside.

13. The Chairman at any Meeting of the Council or of the Institution, when the votes of the Meeting, including his own, are equally divided, shall be entitled to give a casting vote.

14. Persons holding the office of Vice-President shall at all times be entitled to sit and vote with the Council.

15. *Past Presidents and Vice-Presidents.*—All Members who have held the posts of President and Vice-President shall, while their connection with the Institution as Members lasts, be entitled to sit and vote with the Members of Council.

16. *Members of Council.*—Members only shall be eligible for Election as Members of Council at the Annual General Meeting.

17. *Associate Members of Council.*—Associates only shall be eligible for Election as Associate Members of Council at the Annual General Meeting.

18. The Direction and Management of the Institution shall be vested in the Council for the time being, the Associate Members voting with the Members of Council in all cases, except in the decision of questions directly affecting the forms of ships and the construction of their hulls.

19. The Council shall meet as often as the business of the Institution requires, and at every Meeting five Members of Council shall form a *quorum.*

20. The Council may appoint Committees to report to them upon special subjects.

21. All questions shall be decided in the Council by vote; but at the desire, expressed in writing, of any four Members or Associate Members present, the determination of any subject shall be postponed to the succeeding Meeting of the Council.

22. An annual statement of the funds of the Institution, and of the receipts and payments of the past year, shall be made under the direction of the Council, and after having been verified and signed by the Auditors, shall be laid before the Annual General Meeting.

23. The Council shall draw up an Annual Report on the state of the Institution, which shall be read at the Annual General Meeting.

24. It shall be the duty of the Council to adopt every possible means of advancing the Institution, to provide for properly conducting its business in all cases of emergency, such as the death or resignation of Officers, and to arrange for the publication of the Papers read at the Meetings, or of such documents as may be calculated to advance the objects of the Institution.

25. *Treasurer.*—Only Bankers, or Members of Council, or persons who have been Members of Council, and are still members of the Institution, shall be eligible for election as Treasurer.

26. *Auditors.*—All Members and Associates of the Institution shall be eligible for election as Auditors.

27. The Auditors shall have access at all reasonable times to the Accounts of the pecuniary transactions of the Institution; and they shall examine and sign the annual statement of the Accounts before it is submitted by the Council to the Annual General Meeting.

28. *Secretary.*—The Secretary or Secretaries shall be elected by the Council, and shall be removable at the will of the Council, after due notice given. The salary of the Secretary or Secretaries shall be fixed by the Council.

29. It shall be the duty of the Secretary, under the direction of the Council, to conduct the correspondence of the Institution; to attend all Meetings of the Institution and of the Council; to take Minutes of the proceedings of such Meetings; to read the Minutes of the preceding Meeting; to announce donations made to the Institution; to superintend the publication of such Papers as the Council may direct; to have charge of the library, museum, and offices of the Institution; and to direct the collection of subscriptions and the preparation of accounts. He shall also engage, and be responsible for, all persons employed under him, and generally conduct the ordinary business of the Institution.

30. In January of each year the Council shall meet and prepare Lists for the Election of the Council for the ensuing year. These Lists shall be as follows, namely:—

1st. A List of the names of the President, Vice-Presidents, and Treasurer for the ensuing year to be submitted at the Annual General Meeting, for the election in a body.

2nd. Lists for the Election of the Ordinary Members and Associate Members of Council. This List shall contain the names of the existing Ordinary Members of Council and Associate Members of Council, and at least *six* new names of Members, and at least *two* new names of Associates Members of the Institution.

31. No addition shall be made to the total number of the Ordinary Members of the Council until by death or resignation their numbers shall have been reduced below twenty-four, after which their numbers shall be raised to and preserved at twenty-four. And no addition shall be made to the Associate Members of Council until, by death or resignation, their numbers shall have been reduced below eight, after which their numbers shall be raised to and preserved at eight, always exclusive of the President, Vice-Presidents, and Treasurer.

32. Not later than the 14th February in each year, the complete Lists proposed by the Council for the Ordinary Council for the ensuing year shall be printed, and sent to all Members to serve as Balloting Lists. These Lists shall contain, first, the names of the existing ordinary Members of Council, at the time of the preparation of the Balloting List, together with six new names of Members of the Institution (or, if the number shall have been reduced below twenty-four, then so many new names as shall be needed to bring the number up to thirty), and the whole of these names shall be printed in alphabetical order, without any distinction of type. Secondly, the names of the existing Associate Members of Council, at the time of the preparation of the Balloting List, together with two new names of Associates of the Institution (or, if the number shall have been reduced below eight, then so many new names as shall be needed to bring the number up to ten), and

these names also shall be printed in alphabetical order, without distinction of type. From these Lists the new Council shall be elected. Every Member shall be at liberty to strike out any name on the List, but he *must* strike out *six* names from the list of Members of Council, and *two* names from the List of Associate Members of Council.

33. A similar Balloting List (in which, however, the names of the ordinary Members of Council proposed for Election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall be at liberty to strike out any names on that List; but he *must* reduce the number by striking out *two* names from the List of Associate Members of Council.

34. The Balloting Lists may be sent by post or otherwise to the Secretary, so as to reach him before the day and hour named for the Annual General Meeting, or they may be personally presented by the Members and Associates at the opening of the Annual General Meeting.

35. At the opening of the Annual General Meeting the order of business shall be :—

(1.) To read and consider the Reports of the Council and Treasurer.

(2.) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3.) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents, and Treasurer, for election for the ensuing year.

(4.) The Chairman shall next nominate two Scrutineers (of whom one only shall be a Member of the existing or proposed Council), and shall hand to them the Ballot Boxes containing the Voting Papers for the Ordinary Members of Council and Associate Members of Council; and

(5.) The Scrutineers, assisted by the Secretary and his Clerks, shall receive the Ballot Papers which may have reached him, and all others which may be presented by Members or Associates at the Meeting. The Scrutineers shall then retire, and verify the Lists, and count the votes; and shall, not later than the following day, report to the Chairman the names which have obtained the greatest number of votes, subject to the conditions of the Ballot. The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected.

(6.) After the Ballot shall have been taken, and the Scrutineers have retired, the Meeting will proceed to the other business before it.

36. The new Council and Officers shall take office immediately after the close of the Annual General Meeting.

DESIGNATION OF MEMBERS AND ASSOCIATES.

37. Any Member, Associate, Honorary Member, or Honorary Associate, having occasion to designate himself as belonging to the Institution, shall state the class to which he belongs according to the following abbreviated forms, viz. M.I.N.A.; Assoc. I.N.A.; Hon. Mem. I.N.A.; Hon. Assoc. I.N.A.

ELECTION OF MEMBERS AND ASSOCIATES.

38. *Admission of Members.*—Every Candidate for admission into the Class of Members, or for transfer into that Class from the Class of Associates, shall be more than twenty-five years of age, and shall comply with the following Regulation:—

He shall submit to the Council a statement, showing that he has been professionally engaged in shipbuilding or marine engineering for at least seven years in some public or private shipbuilding establishment, or marine engine works, and setting forth the grounds upon which he bases his

claims to be considered a professional Naval Architect or Marine Engineer, and to be admitted as such to the Membership of the Institution. This shall be signed by at least three Members, whose signatures shall certify their personal knowledge of the Candidate, and approval of his statement; or, in the case of persons not British born, the signatures of three Members shall be required, in confirmation of their personal knowledge of the Candidate's scientific reputation.

39. These preliminary conditions being satisfied, the Council shall then consider whether the practical experience and professional attainments of the Candidate are such as entitle him to be brought forward by the Council as a Naval Architect, or Marine Engineer conversant with Naval Architecture. If four-fifths at least of the received votes of the professional Members of the Council are in favour of his application, and such four-fifths constitute a majority of the professional Members of the Council, his proposal for admission shall be submitted to the Members of the Institution (who shall have access to the applicant's statement), at an Ordinary Meeting of the Institution, for them to vote upon, by ballot.

40. *Admission of Associates.*—Candidates for Associateship shall submit to the Council a proposal for their admission, setting forth therein a statement of their claims to be admitted as Associates. Their proposal, if approved by the Council, shall be submitted by them at an Ordinary Meeting of the Institution, for the Members and Associates jointly to vote upon by ballot.

41. The proportion of votes for deciding the election of Members and Associates shall be at least four-fifths of the numbers recorded.

SUBSCRIPTIONS.

42. Each Member and Associate shall pay an Entrance Fee of two guineas, and an Annual Subscription of two guineas in advance; the first Subscription being payable on his election, and all future ones on the 1st day of January of each year. Any Member or Associate withdrawing from the Institution after that date is still liable for the amount of Subscription due on that day.

43. Any Member or Associate may compound for his Annual Subscription, for life, by a single payment of not less than thirty guineas.

44. No person's name shall be entered on the Roll as Member or Associate of the Institution nor possess the privileges of Membership (except it be on the honorary list) until he shall have paid his first subscription or the life-composition, and if the payment be delayed for more than twelve months from the date of his election, the same shall be void, unless the Council otherwise direct.

45. The Secretary shall at the close of every year notify to all Members and Associates whose subscription for that year shall not have been paid, that it will be his duty to report accordingly to the Council, and he shall at the same time furnish the person whose subscription is in arrear with copies of this and of the two following Rules.

46. The Secretary shall before Easter in every year lay before the Council a list of all Members and Associates whose subscriptions for the two previous years shall be still unpaid, and unless the Council shall otherwise direct, the names of those in arrear shall be expunged from the Roll of Members and Associates, and shall not be replaced without re-election in due form. Provided always that the Council shall at any time within two years therefrom have power to dispense with such re-election, and to restore the name to the Roll upon payment of all subscriptions then due and upon cause being shown to the satisfaction of the Council why such subscriptions were not previously paid.

47. Nothing herein contained shall prejudice the right of the Institution to the legal recovering of all arrears of subscriptions up to the date of striking the name off the Rolls.

48. In case the Council should be of opinion that any member, who has been long distinguished in his professional career, from ill-health, advanced age, or other sufficient causes, should not be called upon to continue his annual subscription, they may remit it. Also they may remit any arrears which are due from an individual, or may accept a collection of books, or drawings or models, or such other contribution as, in their opinion, under the circumstances of the case, may entitle the person to be enrolled as a Life Subscriber, or to enable him to resume his former rank in the Institution which may have been in abeyance from any particular causes. These cases must be considered and reported upon by a Sub-Committee named for the purpose.

49. In case the expulsion of any individual shall be judged expedient by ten or more Members, and they think fit to draw up and sign a proposal requiring such expulsion, the same being delivered to the Secretary shall be by him laid before the Council for consideration. If the Council, after due inquiry, do not find reason to concur in the proposal, no entry thereof shall be made in any Minutes, nor shall any public discussion thereon be permitted; but if the Council do find good reason for the proposed expulsion, they shall direct the Secretary to address a letter to the person proposed to be expelled, advising him to withdraw from the Institution. If that advice be followed, no entry on the Minutes, nor any public discussion on the subject shall be permitted; but if that advice be not followed, nor a satisfactory explanation given, the Council shall call a Special General Meeting of Members and Associates, for the purpose of deciding on the question of expulsion; and if two-thirds of the persons present at such Special General Meeting, provided the number so present be not less than thirty, vote that such individual be expelled, the Chairman of that Meeting shall declare such expulsion accordingly, and the Secretary shall communicate the same to the individual.

MEETINGS.

50. Meetings for the Reading of Papers shall be held as frequently, and at such times, as the Council may determine.

TRANSACTIONS.

51. The *Transactions* of the Institution, including the Papers read at the Ordinary Meetings, and Reports of the Discussions by which they are followed, shall be edited by the Secretary, and printed under the direction of the Council.

52. A copy of each Volume of *Transactions* shall be sent free to every Member and Associate.

53. The Secretary, under the direction of the Council, may dispose of the surplus stock of *Transactions* which have been published more than three years, at a price of not less than One Guinea a Volume, provided a sufficient number remain on hand to supply the probable demand of new Members and Associates to complete their sets by the purchase of the back Volumes.

CHANGE OF ADDRESS.

54. Members and Associates are particularly requested to communicate to the Secretary any change of address.

INTRODUCTORY PROCEEDINGS.

The Meetings of this, the Twentieth Session of the Institution of Naval Architects, were held on the 3rd, 4th, and 5th April, 1879, in the Hall of the Society of Arts, John Street, Adelphi.

The opening Meeting was presided over by the Right Hon. Lord Hampton, G.C.B., D.C.L., President of the Institution, who commenced the proceedings by calling upon the late Hon. Secretary, Mr. C. W. Merrifield, to read the Report of the Council, which was as follows:—

REPORT OF THE COUNCIL, 1879.

The Council of the Institution is glad to be able to report to the Members and Associates that the finances of the Institution are in a satisfactory condition.

There is likely to be a small surplus in the course of the present year, at the end of which it is to be expected that the balances in hand will be increased.

On the resignation by Mr. Woolley of the office of Secretary, consequently on his having obtained other employment, Mr. Merrifield, acting under the directions of the House Committee, was kind enough to undertake temporarily the duties of Secretary until a successor could be appointed, and this arrangement was confirmed by the Council. It was thought best to advertise the vacancy. From among a considerable number of applicants, the Council selected Mr. G. Holmes, and, under No. 28 of the Bye-Laws, appointed him Secretary accordingly.

The Council is happy to report that the Library Fund started last year has been subscribed to by Fifty Gentlemen, who have contributed between them £331 9s. Of this sum, £243 2s. was paid to the credit of the Institution in the year 1878, and £88 7s. has been already received this year. £245 1s. 7d. has been expended in furnishing and in augmenting the Library, leaving over a balance of £86 7s. 5d. to be employed in furtherance of the objects for which the Fund was started, in addition to which the Entrance Fees of all New Members and Associates will be devoted to the same purpose.

The Gold Medal, of the value of £25, offered by Mr. R. Carlyle for the best Paper submitted to be read at the Annual General Meeting of the Institution of Naval Architects in 1879, "*On the Construction of the Screw Propeller, its Form, Number of Blades, Pitch in Proportion to Diameter, &c., so as to give the Smallest Amount of Slip with the Greatest Amount of Propulsion for a Ton of Coal,*" has been awarded to A. J. Maginnis, Esq., as the author of the best Paper submitted for the competition.

The Council notices with satisfaction that the popularity of the Institution is steadily increasing, many New Members and Associates having joined since the last Meeting.

The Council has to deplore the loss by death of four Associates of the Institution, viz:— R. Carlyle, Esq., Auditor; P. A. Chessar, Esq., Associate; G. Marshall, Esq., Associate; and T. B. Spence, Esq., Associate.

INSTITUTION OF NAVAL ARCHITECTS.

Statement of Income and Expenditure for the Year 1878.

Dr.

1877.		£	s.	d.	£	s.	d.
Dec. 30th.	To Balance	546	8	11			
	In Secretary's hands	28	16	1½			
					575	5	0½
1878.							
Dec. 31st.	To Cash received :—						
	Annual Subscriptions				1,049	10	10
	Volumes sold				92	13	6
	Admiralty Grant				250	0	0
	Library Fund				243	2	0
	Waste Paper sold				0	10	0
	Rent of Canoe Club				20	0	0
					£2,231	1	4½

Cr.

1878.		£	s.	d.	£	s.	d.
Dec. 31st.	By Cash paid :—						
	Rent of Rooms	238	14	0			
	Cleaning ditto	26	16	9			
					265	10	9
	Attendance at London Meetings	17	12	6			
	Reporting at ditto	45	0	0			
	Abstract of Proceedings	13	15	6			
	Advertising	31	8	0			
					107	16	0
	Printing Vol. XVIII.	341	6	0			
	Engraving, Vol. XVIII.	129	11	6			
	Printing Engravings Vol. XVIII.	23	7	6			
	Binding Vol. XVIII.	46	9	8			
	Cutting Plates for covers (Binding Vol. XVIII.)	5	10	0			
	Despatch of Vols. XVIII. and XIX.	62	7	11			
	Miscellaneous Printing, 1877	98	10	2			
	Lithography, Vol. XIX.	7	15	0			
					714	17	9
	Bankers' Charges				0	8	10
	Stationery, 1877	37	9	7			
	Fire Insurance	2	10	0			
	Drawing up Lease	12	14	6			
	Auditing Books	5	0	0			
	Cost of Exhibits at Paris	7	12	0			
	Petty Disbursements	66	17	5½			
	Furniture and Repairs	196	2	9			
	Removal from 20, John Street	9	19	6			
	Library Expenses	48	18	10			
	Secretary's Salary, with £100 donation	313	14	2			
	Salary of Assistant Secretary	130	0	0			
	Ditto Boys	27	15	0			
	Extra Attendance	14	18	6			
					873	12	4
	Austrian Consulate repaid Cheque				2	2	0
	In Secretary's hands—Current Expenses	19	7	7½			
	Ditto—Annual Subscription	21	0	0			
	Balance in Treasurer's hands	226	6	1			
					266	13	8½
					£2,231	1	4½

C. W. MERRIFIELD, *Acting Hon. Sec.*
J. D'A. SAMUDA, *Treasurer.*

I have examined the above written entries with the books, and find them correct.

JAMES MACKENZIE, *Auditor.*

1*st April* 1879.

The following List of Donations to the Library was read by the late Honorary Secretary :—

"Proceedings of the Royal Society," Vol. XXVII. *Presented by the Royal Society.*

"Minutes of the Proceedings of the Institution of Civil Engineers," Vols. LI., LII., LIII., and LIV. *Presented by the Institution of Civil Engineers.*

"Proceedings of the Institution of Mechanical Engineers for 1878. *Presented by the Institution of Mechanical Engineers.*

"Journal of the Iron and Steel Institute for 1878. *Presented by the Iron and Steel Institute.*

"Journal of the Royal United Service Institution for 1878." *Presented by the Royal United Service Institution.*

"Journal of the Society of Arts for 1878." *Presented by the Society of Arts.*

"Transactions of the Institution of Engineers and Shipbuilders in Scotland," Vol. XXI. *Presented by the Institution of Engineers and Shipbuilders in Scotland.*

"Transactions of the Society of Engineers for 1878." *Presented by the Society of Engineers.*

"Annual Report of the Board of Regents of the Smithsonian Institution for 1876-7." *Presented by the Board of Regents.*

"Lloyd's Register of British and Foreign Shipping for 1878-9." *Presented by the Committee of Lloyd's Register.*

"Lloyd's Register of Yachts for 1879." *Presented by the Committee of Lloyd's Register.*

Underwriters' List of Iron Vessels for 1878-9." *Presented by the Underwriters' Registry of Iron Vessels, Liverpool.*

"Engineer for 1878." *Presented by the Proprietors.*

"Engineering for 1878." *Presented by the Proprietors.*

"Iron for 1878." *Presented by the Proprietors.*

"Revue Maritime for 1878." *Presented by the French Ministry of Marine.*

"Annales du Genie Civil for 1878." *Presented by M. Eugene Lacroix, Paris.*

"Rivista Marittima for 1878." *Presented by the Italian Ministry of Marine.*

"Proceedings of the Royal Society of Victoria," Vol. XIII. *Presented by the Royal Society of Victoria.*

"The Theory of Strains in Girders." By Bindon Blood Stoney. *Presented by the Author.*

The following gentlemen (being duly recommended by the Council) were unanimously elected Members of the Institution :—Mr. Colin Archer, Shipbuilder, Laurvig, Norway; Mr. William Bell, Chief Draughtsman and Manager to Messrs. Walker & Co., London; Mr. John Harvard Biles, Draughtsman in the Department of the Controller of the Navy; Mr. Hans Andreas Blom, Chief Constructor to the Royal Norwegian Navy; Mr. William Winsland Chilcott, Second Assistant to the Chief Engineer, Devonport Dockyard; Mr. John Brown Herreshoff, Senior Partner and Manager in the firm of Herreshoff Manufacturing Company, United States; Mr. Nathaniel Greene Herreshoff, Designer in the firm of Herreshoff Manufacturing Company, United States; Mr. James Humphrys General Manager, Barrow Shipbuilding Company, Barrow-in-Furness; Mr. Edward Inman, Partner in the firm of Inman & Son; Mr. John George Kinghorn, Engineer Surveyor to Lloyd's Register of British and Foreign Shipping; Mr. Fritz F. Maier, Manager to the Imperial and Royal Station for the Improvement of Iron and Steel Industries, Stadt Steyr, Austria; Mr. William Paulson, Surveyor to Lloyd's Register of British and Foreign Shipping; Mr. George Wightwick Rendel, Partner in the firm of Messrs. Rendel Bros., Newcastle-on-Tyne; Mr. George William Sivewright, Chief Draughtsman in the firm of Messrs. Edward Withy & Co., Hartlepool; Mr. Wasteney Smith, Consulting Engineer, Newcastle-on-Tyne; Mr. William Camper Storey, Designer and Constructor for Messrs. Camper and Nicholson, Gosport; Mr. John A. Tobin, Marine Engineer and Draughtsman, United States Navy; Mr. William Walker, Partner in the firm of Messrs. Walker & Co., London.

The PRESIDENT (Lord Hampton) then proceeded to deliver the following Opening Address:—

Gentlemen, in commencing our proceedings of this year with a few opening words, as I have been in the habit generally of doing, it gives me the greatest pleasure and satisfaction to advert to what you have just heard as the contents of the Report of the Council, because you will have learnt from that Report of the Council that the Institution which is now holding its session stands in circumstances of the most satisfactory character, especially in the important and essential item of finance. I think I may say, and for reasons to which I will presently again advert I am peculiarly happy to be able to say it, that I do not think during the period this Institution has existed we have ever met under circumstances of greater prosperity, and altogether of a more satisfactory nature. (Hear, hear.) One item in that satisfaction I think, gentlemen, you must all have felt from the last act we did, which was unanimously to elect a remarkably eligible and desirable list of additional members of this Institution, most of them—I believe I might say without exaggeration all of them—men who are likely to contribute most accurately and most usefully to the great objects for which we are founded.

Gentlemen, I will now advert, if you please, to that which of course is most interesting to us all at the present moment, namely, the nature of the business which in the course of the next three days we shall proceed to transact; and when I say that, I mean, of course, the papers which will be brought before us. There are about the usual number of papers. There is a fair and sufficient number for each of our sittings, and they relate, as they generally do on these occasions, to almost every branch of this interesting subject of naval architecture. But they also relate, and perhaps in a larger proportion than usual, to those subjects which I think are now peculiarly interesting and exciting to the public mind, perhaps never more so, namely, the questions relating to naval armament as well as to naval architecture. These two great subjects of naval armament and naval architecture are so inseparably united that I think I correctly represent the feeling of this Institution when I say that we have always felt that it is impossible, even if it were desirable, which I confess I do not think it is, for us to draw anything like a line of broad distinction between them. We shall this morning commence the proceedings of this session with some papers of some remarkable interest upon the question of naval armament, and those papers will derive additional interest from the eminence and known competence of the pens from which they proceed. Gentlemen, the first paper which will be read to you is a paper on "Armoured Ships of War," by Admiral Sir Spencer Robinson. I need not do more than mention the name of my gallant and distinguished friend, Sir Spencer Robinson, to convince you that the great subject of armoured ships of war could not possibly be treated by a gentleman more competent to illustrate in the most interesting manner the views which he may entertain. The next subject is a paper on "Armour for Ships." That will be read by Mr. Barnaby, the Director of Naval Construction; and these two subjects of armoured ships of war and armour for ships are so closely connected that it has been suggested to me, and I hope you will all concur in that suggestion, that it might be well for the two papers to be read, and then for the discussion to take place on the two papers together; and therefore, subject to your approbation, I shall propose to take that course. Gentlemen, the third paper is one which is not only of very great, but, at this particular moment, is one of very painful interest. It is one by our well-known friend, Mr. Merrifield, on the subject of "Naval Guns." The lamentable event on board the *Thunderer*, of course, you will all feel invests this subject with peculiar interest at the present moment, and we shall all, I am sure, be desirous to hear the views which Mr. Merrifield's paper will express, and we shall be anxious to hear the discussion upon the views which he will so submit to our consideration. It would be premature—indeed I think it would be more than premature, it would be presumptuous, gentlemen, on my part, as a

civilian, if I were now, before hearing that paper, to express an opinion of my own; but the fact of my having been at various times more or less connected with these branches of the service of the country, of course makes me feel, with the rest of the public indeed, but perhaps peculiarly, interested in this subject, and of however little value my opinion may be, I cannot refrain from saying that the tendency —I will not say more than that—of what has taken place with regard to the unfortunate accident on board the *Thunderer*, has been more and more to incline my mind to the opinion, that we shall ultimately find that breech-loading is the best system for the armament of the navy. These interesting questions are not limited, gentlemen, to the three papers to which I have referred. On our last morning I find the first paper will be one "On the Most Powerful Ironclad," by Admiral Selwyn; and here again we shall find a subject of the greatest national interest treated by a gentleman as competent as any gentleman can be, I think, to lay before us sound views upon the subject to which his paper will relate.

Gentlemen, I will not detain you longer by making any further remarks upon the papers to be brought before us; I will only say that on this occasion, as on all previous occasions, I hope I shall be supported, as your President in the chair, by the meeting, in adhering to those rules which we have always acted upon on these occasions, namely, that from a sense of fairness to those who are to follow, the length of the papers shall be limited to twenty minutes, and the length of the speeches in the discussion to ten minutes each, with the limit that the whole discussion is not to exceed an hour. I think that has been our rule hitherto. Of course, if the general sense of the meeting should dictate any deviation from this course the general sense of the meeting will, no doubt, be indicated, and of course it will be my duty to obey whatever may be the decision of the meeting, but, subject to that, I hope I shall be supported in adhering to the rules upon which we have hitherto acted, and which, I think I may venture to say, we have found very conducive to the satisfactory conduct of our proceedings.

Gentlemen, I must turn for a moment to the internal affairs of our Institution. I have already congratulated you, and I think it is a subject of congratulation, that the new members of the Institution are so many and so eligible as they are. The Council notices, in one of the paragraphs of the Report, the increasing popularity of the Institution, as illustrated by the number of members who have lately joined. There is another subject to which I must refer, because it is one relating directly to the internal arrangements of this Institution; it is, that I think there was some degree of complaint some time ago from the idea that the mode in which our various officers have been selected has not been quite so open and so popular as might be desirable, and that has rather borne hardly upon those gentlemen conversant with the objects of this Institution who reside at a distance from London. This question has been taken into consideration by the Council. About a year ago—not so much as a year ago, indeed—a Committee was appointed to consider whether the organisation of this Institution might not be improved, and the result has been that a new code of rules has been drawn up, regulating, in quite a new manner, the election of our Council and of our various officers from time to time. These new regulations have been carefully considered by the Council, and have been adopted by the Council, but the Council are of opinion that they ought to be approved. It is only due to the general members of the Institution that they should be approved by the whole body of the members, and, no doubt, this is the proper occasion for taking that course. I think every member of the Institution on entering the building to-day will receive a printed copy of the new regulations as proposed, and I would suggest to you that to-morrow morning we should meet an hour earlier than usual, that we should meet in this hall at eleven o'clock instead of twelve, and that then these new rules might be submitted to the members of the Institution, to be finally decided upon by them. That is the course which it is proposed we should take on this subject.

Gentlemen, there is another change in our internal arrangements which I think it behoves me now to submit to your consideration. This is the twentieth occasion upon which I have had the honour of presiding over this Institution. (Hear, hear.) Twenty years have passed away since you first did me the honour of electing me as the President of this Institution, and each year I have had the honour of presiding in this chair. Well, gentlemen, twenty years is a long period of time in human life, and one which we must all come to feel more or less, and I cannot help hoping that after that long connection with the Institution, the members of it will be of opinion that the time has arrived when it is desirable, upon many grounds, that I should give up this chair, and resign it to the hands of some younger member. (No, no.) It has been to me, I assure you, a service of very great interest, and I have held it to be one of very great honour. I cannot but acknowledge with respectful gratitude, the constant and kind support which I have received from all members of the Institution, especially from the Council, and those members who have taken the more active part in the management of our affairs, and, gentlemen, it has been by the able and active aid which the friends of this Institution have constantly given to me and my colleagues in the Council, that I think I have seen this Institution rise from infancy to maturity, and in rising from infancy to maturity it has risen, like many other institutions, from small beginnings to its present very prosperous condition, and it is to me a feeling of satisfaction that, if you will kindly allow me, as I hope you will, to retire from the chair that I have filled so long, that whoever will be my successor will find himself called upon to preside over an Institution of the most prosperous, the most successful, and, I may without hesitation say, the most useful and valuable kind.

Since the issue of Vol. XIX. the following gentlemen have been added to the lists of Members and Associates respectively :—Messrs. Colin Archer, William Bell, John Harvard Biles, Hans Andreas Blom, William Winsland Chilcott, John Brown Herreshoff, Nathaniel Greene Herreshoff, James Humphrys, Edwin Inman, John George Kinghorn, Fritz F. Maier, William Paulson, George Wightwick Rendal, George William Sivewright, Wasteney Smith, William Camper Storey, John A. Tobin, William Walker, Members ; and Messrs. William Barr ; Edward Barrow ; Thomas Black, Peninsular and Oriental Steam Navigation Company, Limited; John Darling, Edward J. Hill, John Hughes, Lieut. Hastings R. Lees, Charles William Littleboy, Messrs. Richardson, Duck & Co., Stockton-on-Tees ; Juste Guillaume Lüning, Samuel Joseph Mackie, James McGregor, Shipowner and Shipbuilder ; Bernard Samuelson, M.P. ; Lieut. Theodore Carl A. Sandström, Royal Swedish Navy ; Hallings Tatham, Shipowner ; and John W. Woodall, Associates.

During the same period the Institution has sustained the loss of the following Associates by death : —R. Carlyle, Auditor; P. A. Chessar, George Marshall, W. Froude, M.A., F.R.S., Vice-President; and T. B. Spence.

PAPERS AND DISCUSSIONS.

ON ARMOUR-PLATING SHIPS OF WAR.

By Admiral Sir R. Spencer Robinson, K.C.B., F.R.S. (Vice-President).

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

The scope and object of the paper I have undertaken to read is one which I know you will consider of the greatest importance. It has no less an aim than that of suggesting for your consideration a method of using such armour-plating as we require on the sides and other parts of our ships of war, more effectively than at present. The time that can be given to the reading and discussion of any paper in this hall is, as you all know, extremely brief, and I and others are therefore compelled to dwell only on such parts of the subject as are absolutely necessary for the enforcement of our views. We must leave as sketches and suggestions much that is required to build up a compact and solid argument.

In calling the attention of naval architects and engineers to a mode of giving to the sides of armour-plated ships a more effective defence against projectiles, I beg leave to state that I am by no means forgetful of the convictions firmly held by no inconsiderable or unenlightened members of these professions, namely, that all side armour-plating is a mistake ; and that, at all events, an unsinkable ship may more surely be constructed by a thoroughly well-devised cellular system, in conjunction with an armoured water-tight deck, than by the cumbrous application of heavy armour to the sides of ships.

Now, though I willingly admit that there is something of great weight in such an opinion, I do not myself doubt, that considering all the circumstances of naval warfare, if the side armour can be made to resist the entrance of the projectiles hurled against it, the ship so provided—so encumbered, if you will—must come victorious out of many struggles in which the cellular system would succumb.

We all know that at a certain stage of the duel between guns and armour, the latter had the best of it ; and we are all equally aware that at present this solution has been almost, if not entirely, reversed. I will not deny that it has been entirely reversed, if we simply consider the potential effect of a projectile hurled from the largest gun that can be constructed, against the thickest side armour which it has been hitherto possible to apply to a sea-going ship ; but I think that I am well within the limits of truth when I say that from various causes the maximum potential effect of the projectile will much less often be obtained in practice than is generally supposed

Since we began the use of armour-plating, the destructive powers of artillery have

increased in a ratio almost fabulous. The initial velocities of projectiles, their weight, their size, their energy on impact, their form, their substance, have all added to the difficulty of the defence, have unceasingly called upon the naval architect for larger and larger protective weights, and so have compelled him to use many contrivances for limiting to the utmost the area of the protection he is able to give. The other requirements of modern ships have not assisted him in so sparing weights as at all to reduce the balance, or lessen the necessity for enormous displacements in his designs.

I have been enabled by the kindness of the officers of the Controllers' Department, who have supplied me with all information I asked for, to draw up the following table, which will show at a glance the continued increased proportion of the total displacement absorbed by the protective weights which the naval architect is compelled to provide, as well as the lessening area over which that protection can be made to extend :—

TABLE A.

NAMES OF SHIPS	Displacement at load line.	Total weight of armour, including backing carried by the Ship & Turrets.	Maximum thickness of armour & backing.		Maximum weight of square foot of armour and backing, including skin and frames.		Proportion of displacement to protective armour.	Approximate extent of the protection afforded to the ship by armour, with reference to the water line and batteries.
			On Sides.	On Turrets.	On Sides.	On Turrets		
	Tons.	Tons.			lbs.			
Warrior	9,210	1,354	4½ iron. 18 teak.	None. "	284	None.	6·38	213 ft. of water line out of 360 ft. extending 5½ ft. under water, and 213 ft. of battery.
Minotaur	10,690	2,106	5½ iron. 10 teak.	" "	297	"	5·08	Entirely from stem to stern, and over the whole length of the battery, about 400 ft.
Bellerophon..	7,550	1,273	6 iron. 10 teak.	" "	383	"	5·93	91 ft. of battery, whole of water line, about 300 ft., and 6 ft. below.
Hercules.....	8,680	1,949	9 iron. 22 teak.	" "	511	"	4·45	72 ft. of central battery, 20 ft. at the extremities, all the water line, about 320 ft., extending 6 ft. below.
Alexandra ...	9,490	2,348	12 iron. 12 teak.	" "	645	"	4·00	75 ft. of central battery, 44 ft. of upper battery, all the water line, about 320 ft., and 6 ft. below.
Glatton	4,910	1,965	14 iron. 15 teak.	14 iron. 15 teak.	660	700	2·50	87 ft. of breastwork, 1 Turret, all the water line, about 240 ft. long, extending at lighting draft 6 ft. below.
Devastation...	9,330	2,961	12 iron. 18 teak.	14 iron. 15 teak.	664	726	3·15	153 ft. of breastwork, 2 Turrets, all the water line, about 280 ft. long, and 5 ft. below.
Dreadnought.	10,820	3,666	14 iron. 15 teak.	14 iron. 15 teak.	751	726	2.95	186 ft. of breastwork, 2 Turrets, all the water line, about 320 ft. long, and 5 ft. 8 in. below.
Inflexible.....	11,400	3,553	24 iron. 17 teak.	16irn&stl 18 teak.	1,150	802	3·26	110 ft. of battery, 2 Turrets, and 110 ft. of water line out of about 320 ft. are defended by side armour, the remainder of the water line is protected by an armoured, water-tight deck below the water.

The figures I have given may not be absolutely correct, but they are, I believe, sufficiently so for the purpose of comparison. You are aware that there is some difference in the way the weight of the armour is calculated by various draftsmen, and whether the iron used in certain places is or is not to be reckoned as defensive armour; but I have endeavoured as far as possible in this table to compare different ships one with the other, on the basis of calculation adopted by the Admiralty Constructors; hereafter I shall have to use figures differing from

these, the result of other calculations, but which will not affect the object for which this table was drawn up.

The examples I have given in the preceding table are typical armour-clad ships; they are, they ever will be, monuments of the structural skill and great ingenuity of those who designed them; but we cannot fail to see how enormous are the difficulties which have to be overcome. 284 lbs. weight over a square foot of surface rendered those parts of the *Warrior* to which it was applied impenetrable to the old artillery. 1,150 lbs. on a square foot of surface is insufficient to produce the same result in the *Inflexible* if assailed by modern artillery. In the *Warrior*, the proportion of displacement to armour was 6·38 to 1; in the *Inflexible* it is but 3·26 to 1. In the *Warrior* we had a weight of 1,354 tons to carry which contributed little or nothing to the structure of the ship. In the *Inflexible*, we are carrying 3,553 tons for protection, the greater part of which adds but little to the strength of the structure. It is, I think, only too evident that we cannot proceed in the direction followed from the *Warrior* to the *Inflexible*. I fear that there is little prospect of that reduction of weight which has now become indispensable being effected by any economy in the weights of engines, coals, armaments, or equipments, but there is one most important potential economy opening before us, from which I am sanguine that the greatest advantages may be derived. It is to the manufacturers of steel that the naval architect must turn in his distress, and unless much that was said in this Hall last year was altogether erroneous, to them he will not look in vain, and I for one am hopeful that so large a reduction as twenty per cent. in the weight of a ship's hull, which was then mentioned as possible, may become before long a reality.

Though the earlier experiments of uniting steel and iron in one plate were not successful, the report published by Captain Noble, R.A., of the experiments made on the penetration of projectiles showing a very decided superior resistance in a sound wrought-iron plate over any combination, it does not follow that knowledge more recently gained may not have made it probable that this judgment may be reversed, and that the advantage of breaking up the projectiles on impact may be found greater than the disadvantage of the rapid destruction of the plate which are at present the almost constant results of experiments against steel.

After the Spezzia experiments, I, in common with others, had been led to expect great results from the use of steel of various qualities in the manufacture of armour-plates, but the later experiments scarcely fulfil the earlier promises of success; and steel, or steel and iron, or iron and steel combined in various proportions, can scarcely as yet be received with confidence as substitutes for the best qualities of iron for armour-plates, but I admit that this is debateable ground; and that it is quite possible that not only ships will be built of steel and iron, but that armour-plates may be constructed in a similar manner with considerable saving in weight. Should this be so, we might have in a future *Inflexible* many hundreds of tons to add to her protective defence without any increase of her actual displacement.

Until, however, this possibility has passed into the region of absolute fact, and indeed after that consummation has been reached, the method of distributing the materials of which protective armour is composed requires imperative attention. Let me, as briefly as I can,

describe what *is* done in our armour-plated ships, and then what I think ought to be done, and why.

The original idea of armour-plating, as exemplified in the *Warrior*, was simply to attach to that part of the ship intended to be protected, a plate of iron (four and a half inches thick) in contact with two thicknesses of teak (eighteen inches), secured by strong bolts to the skin of the ship, this cushion of wood and iron combined resting at its lower part on a recess formed by the frames of the ship, and a strong iron shelf-piece.

Notwithstanding that this system was for the time perfectly successful, its defects, as shown by the rapidly increasing powers of artillery, led to many attempts to improve this arrangement, and after various experiments, more or less successful, especially those made against structures known as Clarke's and Chalmer's targets; the Admiralty decided on the modification adopted in the construction of the *Bellerophon*, which I believe, as long as the armour-plate is in one thickness, is in use up to the present time. (Plate I., figs. 3, 4.)

The *Bellerophon* carried armour-plates six inches thick, resting on a teak backing ten inches thick, which formed a cushion in front of the skin of the ship consisting of two thicknesses of iron, three-quarters of an inch each, but at intervals of about two feet vertically along the armoured part of the ship a horizontal plate of half-inch iron was introduced, the outer edge of which strips alternately came into contact with the inner face of the armour-plate or remained one-eighth or one quarter of an inch short of doing so. The inner edge of this strip was effectively secured to the skin of the ship.

Thus a very material improvement in the structural strength of the ship where it carried armour was effected, as well as a great addition to the power of resisting projectiles. The idea of stopping the edges of the strips behind the armour, short of actual contact with it, was to prevent the armour being cut or scored when driven against the strip by the impact of a projectile. As a matter of fact, however, sooner or later this scoring did take place, and in process of time the *Bellerophon's* structure, though impenetrable when it was fired at, ceased to give adequate protection against the ever-increasing power of modern projectiles.

Recourse was had to thicker plates of iron, deeper cushions of woods, and thicker strips of horizontal iron between the logs of teak. This arrangement, adopted in the construction of the *Hercules* (Plate I., figs. 3, 4), where nine and eight inch solid plates backed with twelve inches of teak, and supplemented by filling vertically between the frames of the ship ten inches of teak, a double cushion of wood, and inner skin behind them, proved perfectly impenetrable to projectiles at the end of 1865. I do not believe that in other ships the double cushion and second inner skin has been adopted; with this exception, so long as a single plate of iron was used, the distribution of armour and backing has only varied by the increased thickness necessary for protection. It will be seen by the table that we have gone from the square foot of the *Warrior*, weighing 284 lbs., to the square foot of the *Hercules*, weighing 689 lbs. Still driven forwards by the inexorable artillerists, we find fourteen-inch iron plates backed by fifteen inches of teak, gradually swelling the weights of a square foot of surface of the ship's sides from 645 lbs. to 751 lbs., until at last, the single iron plate of the requisite thickness being too difficult to manufacture, recourse was had to two or more thicknesses of armour with divided backing, and we

have in the *Inflexible* reached twenty-four inches of iron, seventeen inches of teak, and a weight per superficial foot of 1,150 lbs.

I have carefully analysed the various experiments made upon these structures, or the targets intended to represent them, and I have drawn up a Table B, giving the several peculiarities of these constructions with reference to their size, distribution of armour, weight, the amount of pounding they received, and the general effect of their powers of resistance. I have taken as types of armour-plating adopted, or to be adopted, the following, namely, *Bellerophon ; Hercules ;* No. 33, a compound structure ; No. 34, a target representing a turret with a single thickness of armour-plate ; No. 35, a turret with two thicknesses of armour-plating ; the turret of the *Glatton*, in place in the ship ; No. 40, a compound target ; No. 41, a compound target ; the Italian targets fired at Spezzia, and structures known as the Gibraltar and Millwall Shields.

In this table I wish particularly to call your attention to a column representing the total energy of the projectiles received by the target, and the energy divided by the number of surface feet in the target ; this total energy may fairly represent, where perforation is not effected, the racking and tearing to pieces strain on the structure ; the column which gives the energy on impact of any projectile, per inch of the shot's circumference, will fairly represent its penetrating power, varying of course with the form and material of which the projectile is composed. The whole together will give a fair comparative idea of the resisting powers of the several structures.

I have not thought it necessary to give the distance of the target, the weight of powder and its quality, the diameter, forms and weight of each projectile used ; they are all contained in the figures representing the total energy of the shot on impact,—in fact, the *vis viva*, or $\frac{W. V^2}{2g}$, but these particulars for each target will be found tabulated in an appendix attached to this paper. The Palliser pointed ogival-headed projectile is, however, a much more powerful weapon for forcing its way through armour-plates than any other, the Whitworth steel excepted ; all the targets fired at since its adoption have been much more severely tried than the older ones, and mention is made wherever they have been used.

I need only further remark that finding the actual weights of the *Bellerophon* and *Hercules* targets given in the official reports of the trials of these targets, as well as the calculations on which they were based, I have computed the weights of the other targets given in Table B on the basis there laid down, and though the results are not the same as those furnished by the Admiralty for the structures which the targets represent, yet, as I have given in that table the total weights for each target as it stood upon the ground independent of its supports, I think for the purposes of comparison my figures will be found sufficiently correct.

[TABLE

Table B.

Date of experiment.	Names of the ships, or numbers of the target structures experimentalised upon.	General description of the targets fired at.	Approximate: Weight of the target fired at, per superficial foot.	Approximate: Number of superficial feet of structure fired at.	Total energy of the projectile which struck the target $\frac{W.V^2}{2g}$ in foot tons.	Energy per square foot of surface in foot tons.	Maximum energy of any projectile, per inch of the shot's circumference in foot tons.	General Remarks.
			lbs.			Foot tons.	Foot tons.	
1863	*Bellerophon.*	6 in. armour plates, backed by 10 in. of teak, frames, skins, horizontal edge plates, &c.; plates in one thickness.	393·5	176·37	17,721	100	78.92	The 6 in. plates were twice completely perforated, but the structure was not. No Palliser projectiles used.
1865	*Hercules.*	9 and 8 in. solid armour plates, behind which 12 in. of teak, two skins $\frac{3}{4}$ of an in. iron each, the usual frames, between which were fitted 10 in. of teak, behind which again 18 in. of teak in two thicknesses, a covering plate of $\frac{5}{8}$ in. iron, and angle irons to hold the structure together.	9 in. portion, 689. 8 in. portion, 652.	Total area, 145·2. 8 in. 72·6. 9 in. 72·6.	Total of the whole target, 64,499	8 in. 513 9 in. 378	168·5	No sound part of the target was penetrated; the 9 in. and 8 in. plates were however perforated, but not the structure, by chilled projectiles from 13 in. M. L. R. gun.
1871	No. 33.—Thick portion.	8 in. solid plate, backed by 6 in. of teak, and 5 in. solid plate backed by 6 in. teak, $1\frac{1}{2}$ in. skin, frames, &c.; a sandwiched target.	684	Not recorded.	25,689	Not recorded.	175·3	The target was completely penetrated through and through by the 10·92 projectiles, weight 533 lbs., brought against it.
1871	No. 34.—Bent to represent a portion of a turret.	14 in. solid armour plate, behind which 15 in. of oak, horizontal edge plates, skins $1\frac{1}{4}$ in. thick, frames, &c.; total thickness, $30\frac{1}{4}$ in.	736	102·5	Direct hits 53,117	518	182·9	Very much disintegrated; a match with but little margin for the gun brought against it.
871	No. 35.—Bent to represent a turret target; a sandwich.	8 and 6 in. solid plates, with two thicknesses of 9 in. and 6 in. of oak, inner skin, ribs, horizontal plates, and total thickness as No. 34.	736	102·5	Direct hits 51,656	503	185·5	The practical protection of the inner skin the same as in No. 34; the depth of penetration was greater, however, in most cases.
1872	H.M.S. *Glatton*, with turret in place.	14 in. solid plate, backed as in No. 34, and apparently resembling it in all respects: the usual covering plate was attached to the frames.	736	On a surface of the turret, about 12ft. long, by 7 ft. 2 in. high.	13,500 Two shot, one not a direct hit.	160·7	181·0	No through penetration; the head of the shot got through the 14 in. plate, and backing within 2 in. of the skin. No damage to turning gear, guns, or live objects inside.
1876	No. 40.—First experiment.	Three plates of $6\frac{1}{2}$ in. iron, backed by two layers of wood 5 in. thick; skin, &c. as usual.	963	69	11,408	165	290	Projectile went through, setting teak backing on fire: target and gun just equal.
	No. 40.—Second experiment.	Four plates of $6\frac{1}{2}$ in. iron, backed by three layers of wood, and as above.	1253	69	13,911	185	335	Second experiment.—Did not penetrate more than 20 in. into the structure.
1877	No. 41.—First experiment.	Four 8 in. plates, three layers of 4 in. teak between, and backed as above.	1520	160	26,380		526	Did not go through penetration 25 in.
	No. 41.—Second experiment.	The same target was fired at with the 80 ton gun, chambered to burn more powder, and give greater velocity to the projectile.	1520		29,607		590	Did not go through; penetration 27 in.

Date of experiment.	Names of the ships, or numbers of the target structures experimentalised upon.	General description of the targets fired at.	Approximate: Weight of the target fired at, per superficial foot.	Approximate: Number of superficial feet of structure fired at.	Total energy of the projectile which struck the target $\frac{W.V}{2g}$ in foot tons.	Energy per square foot of surface in foot tons.	Maximum energy of any projectile, per inch of the shot's circumference in foot tons.	General Remarks.
1868	Millwall shield, intended for a fort, with an embrasure, or port-hole.	Target in two parts, one 9 in. solid plate, one 6 in. solid plate above it, a port-hole cut out of the middle of each plate. The 6 in. plate covered for a considerable portion of its length by three 1 in. plates, the whole backed by two sets of hollow 7 in. stringers, one vertical, the other horizontal, the hollows filled with teak, two $1\frac{1}{2}$ in. plates for skin in front of the vertical stringers, or frames, and a covering plate $1\frac{1}{4}$ in. thick behind all.	The 6 in. 558. The 9 in. 678.	6 in. 24·3 Total 97ft. 9 in. 48·6 6 in. + 3 in. 24·3	Total on whole target 66,035 6 in. 18,476. 9 in. 35,960. 6 in. + 3in. 11,598.	On whole target, 679. 6 in. 760. 9 in. 740. 6 in. + 3in. 477.	152·3 153·93	This target was perfectly uninjured by fourteen rounds from guns below the calibre of 12 in., after which the 6 in. and the 9 in. plates received two shots from 12 in. gun, projectile 600 lbs. Where the port-hole had been cut the projectile went through, but in the other parts both 6 in. and 9 in. resisted penetration.
1867	Gibraltar shield.	Consisted of front plates $5\frac{1}{2}$ in. thick, supported by plates 5 in. thick, and that again by plates $1\frac{1}{2}$ in. thick, no wood between the plates, and total thickness 12 in. of iron.	458	98·2	27,103		110·3	The 9 in. 12 ton gun was more than a match for this target in its weakest part, but it was capable of resisting this projectile in its strongest part. Four trials were made against this shield, and it was repaired in the intervals.
		These figures refer to the first and second trials only.						
1868	This shield was altered and strengthened, and subjected to further trials.	As this target was manifestly too weak for the projectiles which could be brought against it, no lesson of importance can be learnt from observing its details any further.						
1877	Series of targets tried at Spezzia.	These targets consisted in their main features of 22 in. of iron in one or more thicknesses, and 19 in. of wood with horizontal edge, stringers, skin, &c. Solid steel plates of 22 in. in thickness were tried, and solid iron plates of that thickness. Cast iron backing was also used, with sandwiches of wood and without, but it was observed in all cases that the width of the plates was too small.	1041	These targets in all cases resisted perforation by 10 and 11 in. projectiles, though, when sufficient velocity was given to the 2,000 lb. projectile from the 100 ton gun, they were either perforated or destroyed. The solid steel plate, 22 in. thick, proved an exception to the perforation, though the plate itself was shattered to pieces. As a matter of fact, none of these targets had a resisting power equal to No. 41, though the *vis viva* brought against some of them was upwards of 31·100 tons.				

Before considering in detail the lessons which this table appears to convey, it would be as well to sum up the broad facts which the experiments against targets have established.

These are, that the resistance offered by solid iron plates of good quality to projectiles varies very closely as the square of their thickness—only, when the thickness of the plates is small, and their character therefore perfectly homogeneous, that solid plates have considerably greater resistance than divided plates, the total thickness of the iron being equal, if the divided plates are thin, but that this advantage decreases as the thickness of the plates increases, and

in great thicknesses seems to disappear; that wood backing, by itself, affords little or no support or additional strength to an iron plate when struck by a pointed shot, but that rigid backing such as is given by good wrought iron is of great advantage to the plate. The Admiralty constructors have availed themselves of this principle by the horizontal stringers or plates introduced into their wood backing; and proofs of its value are to be found in every target of which these formed a part.

There are no satisfactory or conclusive experiments on the resistance afforded by wood backing, but there is a great power of resistance, beyond all doubt, in closely-packed masses of wood, such as the forty inches of wood used in the *Hercules*, and great support is afforded by an inner skin of iron. There are, however, several records of wood backing being set on fire, when used, as in the *Warrior* targets, eighteen inches thick, behind an eight-inch armour-plate.

Cast iron has been tried as a backing, and also granite: they have invariably failed.

Projectiles of steel or chilled iron with pointed ogival heads are the most favourable for penetration. Such projectile has a power of penetration directly in proportion to the $\frac{W.v^2}{2.g}$ on impact, and inversely in proportion to its diameter, whether this energy is made up by velocity or by weight.

There is one other fact about the penetration of armour-plates to which I must refer, that is, that when an armour-plate is well able to resist much more than the *vis viva* of any projectile, the latter will penetrate far less than it would if its power was nearly equal to effect penetration. Accordingly, under such circumstances, the measurements of indent and penetration are apt to be misleading.

For the sake of convenience, I have, as far as possible, when comparing the effects produced by the various projectiles hurled against targets, reduced the *vis viva* due to their several diameters, weights, and velocities to those which a Palliser projectile of 11·92 inches diameter and a weight of 600 lbs. would have required to equal their penetrative effect, and thus I shall state the effects produced in terms of a 600-pounder, though, in some cases, no such projectile was fired against the structures.

Table B shows us that there are four methods of armour-plating in use:—

(1) In which the armour consists of a solid iron plate in one thickness with more or less wood behind it, an inner skin and ribs behind the plate, and horizontal plates of thin iron between the thicknesses of wood. This system is represented by the *Bellerophon*, *Hercules*, *Glatton*, the side of the *Devastation*, No. 34 target, and some of the Spezzia targets.

(2) In which the armour consists of two or more thicknesses of iron plate, with wood more or less thick between them, and the usual skin and ribs. These structures are generally known as sandwich; and they are represented by No. 33 target in its thick part, No. 35, No. 40, No. 41, and several of the Spezzia targets.

(3) In which the armour consists of an outer plate resting on an inner iron plate, or cast-iron block, either immediately in contact with the front plate or separated by a thickness of wood. The Gibraltar shield, a War Office shield constructed in 1870, is an example in England, and two targets at Spezzia are foreign adaptations of the principle.

(4) In which an armour-plate of one thickness is supported throughout at its back by hollow rolled iron stringers filled with wood, for which system Mr. Hughes holds a patent. It differs very materially from that adopted generally in our ships. The weight of iron used is much more incorporated into the structure of the ship. The ship's frames or ribs behind the portion intended to be armour-plated are of this shape, the hollow being filled with wood.

Behind these frames or hollow vertical stringers is a thin covering-plate of iron. In front of the vertical stringers or frames comes the skin of the ship, in two thicknesses. In front of this, again, the horizontal hollow stringers, of the same depth as the vertical stringers or frames of the ship, also filled with wood, and outside of this the armour-plate. Of course, the thickness of each part of this structure varies with the amount of protection required. It is to this particular mode of construction, and to comparison between its resistance under fire and those obtained by other systems of backing and construction, to which I desire to call your attention. In addition to the great augmentation of strength obtained by the use of the vertical hollow stringers for the frames of the ship, and to the great support given longitudinally by their application horizontally behind the backing, it will be seen how much more of this backing is of a rigid nature than can be given by logs of wood. It is only the rigid portion of any backing that gives support to the armour-plate; and the Admiralty constructors have most wisely and skilfully availed themselves of this principle, by the introduction of horizontal stringers or edge plates between the logs of wood forming the backing. This principle, adopted in the construction of the *Bellerophon*, *Hercules*, and all subsequent ships, has been completed and carried out to its fullest extent by Mr. Hughes. He has diminished the unsupported area of the back of the armour-plate, while he has left interstices filled with wood vertically and horizontally to cushion and absorb the blow, and while his backing is more rigid and less soft than the old system, it preserves a large amount of elasticity and power of absorbing and diffusing the shock before it reaches the inner skin.

The drawing (Plate I., fig. 1) shows the system applied to a six and nine inch plate on a ship's side, as it was applied in the Millwall target. In considering the latter, however, when the results of firing at it are related, it must be observed that a portion of the hollow stringers behind the six-inch plate were designedly not filled in with wood.

Figs. 3 and 4 are sections and plans of the *Bellerophon* target. Figs. 5 and 6 are sections

and plans of the *Hercules* target. They will be used to compare the support given to six and nine inch plates by this system with that afforded by the system of Mr. Hughes.

On referring to Table B we find that the *Bellerophon* target was 176·37 ft. in superficial area; that it received various projectiles, none of them of the pointed Palliser form. The total energy of the projectiles on every superficial foot of its surface was 100 foot tons. Two projectiles passed through the six-inch plate, but did not perforate the structure, and reducing them to the terms of an 11·92 projectile, or 600-pounder, the most damaging effect was equal to what a *vis viva* of 2,417 foot tons would have produced from a 600-pounder. The weight of this structure without support was 393·5 lbs. per superficial foot; no port-hole was cut through it, which therefore received all the advantages which great size and perfect homogeneity can give to a target, a matter of very great importance in reckoning up its powers of resistance.

The Millwall shield, on which the six-inch armour plate was tried, being intended for a fort, was weakened by an embrasure, and its supports were so much mixed up with the target proper that a very accurate comparison of weight is difficult to make. Its surface was 98 superficial feet; it was divided longitudinally into two plates, a six-inch and a nine-inch plate, out of equal portions of which a port-hole was cut, but on the proper right of the target three one-inch plates were fastened on to the six-inch plate extending beyond the aperture of the port, and occupying the whole depth of the six-inch plate; there was thus more of nine inches of iron on the upper portion of the target than of six inches, and the six-inch plate thus derived such support as the connection with the heavier portion of the target could give it. However, the area of the six-inch plate which it is my object to compare with the *Bellerophon's*, was only 24 ft. 3 in., superficial feet, and its weight per foot, deducting supports, was about 500 lbs. It received from pointed Palliser projectiles a total energy of 18,476 foot tons, or 760 foot tons per superficial foot, sevenfold more than the *Bellerophon*; the maximum energy of the 11·92 in. projectile was 5,705 foot tons, with a velocity of 1,171 ft. in a second, which according to the tables calculated by Capt. English, of the Royal Engineers, would represent the resistance offered to complete perforation by an unbacked solid plate 12·3 in. thick, showing the support given by the hollow stringers to be at least equal to an iron plate of 6·3 in. thick.

I must pass on to the comparison of the Millwall nine-inch plate with the *Hercules* and other targets, for, however greatly superior the Millwall six-inch plate showed itself, the days of six-inch armour are long passed.

The *Hercules* structure was peculiar, and was not, I believe, adopted in any subsequent ship; its thickness was enormous, averaging more than four feet. Its nine-inch plate was backed by similar edge plates, or horizontal stringers, only of greater thickness, than those used in the *Bellerophon*, and the interval between the ribs or frames of the ship was filled up by vertical logs of teak in addition to the horizontal wood backing and the two skins. But besides this there was added to this ship, and to this ship alone, a thickness behind the frames of eighteen inches of teak, a covering plate, and set of angle irons or inner frames to support the wood. This target might well be described as the strongest ever fired at, up to that date, and as impenetrable in a sound part to any projectiles which could then be brought against it.

Confining myself to the nine-inch plate, and reducing the blows it received from a 12·95 projectile to the standard already described, that of an 11·92 projectile of 600 pounds, it appears that the nine-inch plate was penetrated, but not the structure, by a velocity from that projectile of 1,234 ft. in a second, which would be just sufficient to completely perforate a 12·6 in. unbacked solid plate. I may just state that the nine-inch plate of the *Hercules* had an area of 72 ft. 6 in., and weighed per superficial foot of its surface 689 lbs. It received a *vis viva* of 378 foot tons per every superficial foot. Its resistance was considered by the officers directed to report on it as being so satisfactory that they advised a consideration of the propriety of reducing the nine-inch plate to an eight-inch plate for the future.

The area of the nine-inch plate in the Millwall target was 48ft. 6in. superficial feet, out of which half of a port-hole was cut. It received a total energy from projectiles of 740 foot tons per superficial foot, nearly double what the *Hercules* received, and the weight of this part of the structure per superficial foot was 678 lbs. The structure was not penetrated.

As neither system was tested to the extent of complete perforation I am not in a position to compare directly the resistance of the two structures, but I am obliged to have recourse to an indirect comparison. The six-inch Millwall structure, struck by a 600 lb. projectile with a velocity of 1,171 ft. in a second, was not completely penetrated in a tolerably sound part, though according to Captain English's tables this velocity was just able to completely perforate a 12·3 in. unbacked armour plate, and as the same stringers were placed behind the nine-inch plate, that must have received the same support as the six-inch plate received. This has been shown to amount at least to 6·3 in. of armour-plate, and it follows that the resistance of the nine-inch part of the Millwall shield was at least equivalent to that of an unbacked plate 15·3 in. thick: the least velocity from a 600 lb. projectile which would perforate such a plate being 1,535 ft. in a second.

There is another very strong though also indirect testimony to the fact of the nine-inch plate in the Millwall target resisting perforation to the same extent as a fifteen-inch unbacked plate would do. In 1870, a shield designed by the War Office, admirably answering its purpose, and displaying very great ingenuity of design, was erected at Shoeburyness. It was subjected to most severe pounding. This target consisted of three five-inch armour-plates, five inches apart, the space between them filled in with iron concrete; behind the last was a skin, or case, and a most skilful system of supports. The whole weight of this structure without concrete (it measured 98 superficial feet) was about 35½ tons, with the concrete 56 tons, The first four shots fired at it were from a twelve-inch M. L. R. gun; the projectile of 11·92 in. diameter, velocities varying from 1,177 to 1,190 ft. in a second. The projectile with these velocities was quite unequal to perforate the target; the penetration obtained by the three direct hits from this gun was 23·2 in., in, be it observed, quite a new uninjured structure.

After the Millwall had received projectiles whose total energy was upwards of 43,000 foot tons, it was subjected also to four rounds from the twelve-inch gun (diameter of projectiles 11·92), with velocities varying from 1,154 to 1,181 ft., two of these against the six-inch plate, and two against the nine-inch plate. Practically the *vis viva* against both was the same, and the penetration into this battered nine-inch plate was with the low velocity 15½ in, and with the high

velocity 23·2 in. Its resistance was clearly not unequal to the Engineers' shield, where the total thickness of iron plates was fifteen inches, supported elaborately and heavily by concrete and iron. And this comparison corroborates the theory, that the nine-inch plate backed by Mr. Hughes' system is capable of as much resistance as a 15·3 inch unbacked plate. If this be so, and if the tables I have quoted can be relied on, the nine-inch Millwall plate showed very superior resistance to the thick part of No. 33 target, and also greater powers of resistance than Nos. 34 and 35 targets, and than the *Glatton's* turret *in situ.*

The first of these targets, No. 33, was a sandwich, consisting of eight and five inch iron plates, each plate backed by six inches of teak, with ribs, inner skin, and edge plates, &c. It was completely perforated by a 600 lb. projectile with a velocity of 1,173 ft. in a second (reduced from the 10·92 projectile actually fired at it). By the tables I have already quoted the lowest velocity from such a projectile which would enable it to completely perforate a thirteen-inch solid plate, was 1,165 ft.; a very few (eight) feet more caused it to go through thirteen inches of iron, and all the backing, skin, &c. This result was not very favourable to the sandwich system.

No. 34 target was faced by a fourteen-inch plate; it had fifteen inches of backing, skin, ribs, &c., and it was bent to a radius of 15·7¼ inches to represent a turret. Reducing the projectiles which struck it to the standard 600-pounder, this target received a blow from it with a velocity of 1,283 ft. in a second, just sufficient to completely perforate an unbacked plate of 13·3 in. It did not get through the structure; the point of the shot got close, however, to the inner skin, which was severely cracked and bulged.

No. 35 was a sandwich target, of eight and six inch armour-plates, backed respectively by nine and six inches of wood, in every other respect exactly like No. 34. It was struck by a projectile, reduced to the 600 lb. standard, with a velocity of 1,293 feet in a second, a velocity just sufficient to perforate completely an unbacked plate 13·35 inches thick. It did pass completely through the eight-inch plate, and penetrated deeply into the six-inch plate, the head of the shot getting through it but stopping short of the inner skin. Both these structures were very severely shaken and much disintegrated. The official reports said that these targets were able to resist the guns brought against them, but with little or no margin, and their powers of resistance were thought to be practically equal. To perforate completely a fourteen-inch unbacked plate a velocity from the 600-pounder of 1,390 feet in a second would be required, which exceeds by 100 ft. the maximum force brought against either of them; but judging by the effects produced by a velocity of 1,283 feet in a second, the resistance of either of these structures to complete perforation could not have exceeded that offered by a fourteen-inch unbacked plate. The last example I can use as a comparison with the Millwall shield is the trial made against the *Glatton's* turret in place. When fired at I believe it exactly represented No. 34, being faced with the fourteen-inch solid plate, backing, &c. The projectile from the 600-pounder fired at the spot selected had a velocity of 1,275 feet in a second; it penetrated the fourteen-inch plate, and its head was within two inches of the inner skin, but it did not perforate the structure. The turret did not offer any greater resistance to penetration than Nos. 34 and 35, but it was very much less disintegrated than either. From these facts we may deduce with confidence that the

nine-inch Millwall plate, backed by the seven-inch stringers, is much superior in its resisting power to the fourteen-inch plates as ordinarily backed, whether those plates are in one or two thicknesses.

The tables show how much more pounding fell on each square foot of the Millwall than on any of the other targets.

The *Hercules* received 378 foot tons of projectiles on every superficial foot of the target. Number 34 received 518. Number 35 received 503. The *Glatton* received two shots only in a limited space, while the Millwall, on the nine inch plate, received 740 foot tons on every superficial foot of its surface, and after all this pounding was in so good a state as to be used again to try the effect of some Swedish projectiles from Finspong, and then was finally fired at by seven 11·92 inch projectiles to break it up, which was not effected without much difficulty.

I do not profess that the standards of comparison I have used can be absolutely correct; they are given as approximations only.

Before leaving this part of the subject, I ought to say that I have endeavoured most carefully to abstain from crediting the system of Mr. Hughes with results of too favourable a nature. That gentleman is persuaded from experiments he has made with falling weights that the hollow stringers offer much greater resistance than I have deduced from the experiments and comparison which I have brought before you. It is certainly true that my comparisons are derived from the Millwall shield, after it had received an extraordinary amount of pounding from projectiles—when, in fact, it was difficult to find any really sound part of the target on which to plant a twelve-inch projectile. I would ask anyone to look at the photograph on this table, representing that target before it received four blows from a 600 lb. projectile, especially to notice the small area of the six-inch plate which received two such blows, and I appeal with confidence to his judgment whether, as complete penetration was not effected by a 600 lb. projectile, with 1,171 feet of velocity, through so battered a structure, I should not be justified in placing its resistance much higher than that of an unbacked solid plate of 12·3 inches, which the velocity above given would just be sufficient to pass completely through. I am persuaded that on a sound target the six-inch plate backed on Mr. Hughes's plan would offer quite as much resistance to complete penetration as an unbacked solid plate of thirteen inches, and that the seven-inch hollow stringers may add as much to the resistance offered by any armour-plate behind which it is placed, as is equivalent to a solid plate 7 inches thick.

I forbear from saying that this is proved. I put it forward as an opinion which there are good grounds for entertaining.

For the purpose of comparing the resistance offered by these various methods of backing I need go no further. The Gibraltar shield was in every way so inferior to the powers of modern guns, and was so ill adapted for use in a ship, that it need not be considered. I have, however, inserted the results obtained from it, in Table B, where they may be consulted. The officers' report on the subject said that in its *strongest* part its resistance was about equivalent to that offered by a ten-inch unbacked solid plate.

One of the important merits claimed by Mr. Hughes for his system is that for equal

resistance it is much lighter than the ordinary system of defensive armour. If we take the turret of the *Devastation* as equivalent to the Millwall nine-inch plate, in its sound state, as to its powers of resistance;—and I purposely take the *Devastation's* turret, which I conceive to be stronger than the *Glatton's* as originally constructed, the latter having been strengthened since it was fired at;* and also because I think the resistance of the *Glatton's* turret when fired at was not equal to the nine-inch Millwall plate and backing, while that of the *Devastation's* would probably have been nearly so;—then the comparison of weights will be as follows, taking the strongest part of each structure:—

"Devastation."		lbs.	Millwall, 9-inch Plate.		lbs.
Armour-plate	14 × 40 =	560	Armour-plate	9 × 40 =	360
Teak, 18 inches	1·5 × 42 =	63	Two ¾-plates	1½ × 40 =	60
Skin, 1½ inches	× 40 =	60	Vertical frames, hollow stringers		152
Frames, fastening, covering plates, &c.		68	Teak		36
			Covering plate, 1¼		50
			Bolts, &c.		20
Per superficial foot		751	Per superficial foot		678

There would thus be a gain in the Millwall system of 73 lbs. per superficial foot for equal resistance, or 3·26 tons in 100 superficial feet, so that if we had a surface 150 feet long by 15 feet high to plate, we should save by the adoption of the Millwall system about 72 tons in weight, add very considerably to the structural strength, and immensely diminish the chance of setting fire to the wood backing under a powerful shell fire.

It is of course obvious that, as the power of projectiles has enormously increased since the Millwall shield was constructed, there are improvements in its construction which have naturally occurred to Mr. Hughes, without any abandonment of its principle; I will just endeavour to show, by one or two examples, how the defence on this principle may contend with the fearfully increased power of the attack with better prospects than at present. We have, as is clearly shown in Table B, been induced to abandon single armour and adopt sandwich structures. One of these, known as No. 40, consisted of three 6½ inch plates, with five inches of teak between them, making a total of 19½ inches of armour plating, and 10 inches of wood with (if applied to a ship) the usual frames, edge strips, inner skin, &c. This was fired at by a projectile of 815 lbs. in weight, 12·5 inches in diameter, a velocity of 1,421 feet in a second, and a total *vis viva* of 11,408 foot tons; the projectile passed through all, and set fire to the teak backing. This projectile was stated in the official report to be about equal to the resistance offered by the target, but as it got through it, it had clearly somewhat the better of it. According to a formula recently given by Captain Browne, late of the Royal Artillery, as one which is practically used to solve these questions, the *vis viva* of this shot was just able to penetrate completely a 19·41 inch unbacked plate, and reckoning as he does that three 6½

* This is a mistake. The *Glatton's* turret, I am assured, was not strengthened after being fired at. Her turret and the *Devastation's* are of the same weight, and in all respects are represented by target No. 34. The weight given in the text for the *Devastation* turret should be 726 lbs., and it would not, therefore, in my judgment, offer as much resistance to projectiles as the Millwall 9-inch plate with its stringers.

plates are equal to about 17½ inches of solid iron, the result was in accordance with theory. This target was strengthened by the addition of another sandwich of 6½ inch iron and five inches of wood, and resisted complete penetration by the same projectile with a velocity of 1,525 feet in a second. Now let us see how Mr. Hughes might have dealt with a projectile just capable of penetrating completely an unbacked plate 19·41 inches thick. He could apply a twelve-inch armour plate, two hollow stringers of nine inches interfilled with wood, two plates one inch thick for skin, and a covering plate of 1¼ inch iron. The comparative weights would be as follows, per superficial foot :—

Sandwich.	lbs.	Millwall.	lbs.
Armour-plates	780	Armour-plate	480
Wood	38	Two 9 inch stringers	194
Skins (1½ inch)	60	Plates, 2 in.	80
Frames, fastenings, covering plates, &c.	87	Wood	55
		Covering plate, 1¼ in.	50
		Bolts, &c.	30
Total	965	Total	889

If this arrangement offered only an equal resistance in proportion to that shown by the Millwall nine-inch plate in its battered and injured condition, it would be equal to a 20·1 inch solid unbacked plate. The respective weights of the two structures, per superficial foot, would have been : sandwich, 965 lbs., which could not have kept out the shot ; Millwall, 889 lbs., which would do so with something to spare ; the sandwich requiring a total thickness of 43 inches, the Millwall 36½ in. Proceeding to No. 41 target and the 80 ton gun—a very extreme example—we find a sandwich composed of four eight-inch plates with three layers of four-inch teak between ; the usual frames, skins, &c., must have been added if this structure had been applied to a ship. Against this a projectile was fired 14 inches in diameter, 1,700 lbs. in weight, with a *vis viva* of 29·607 foot tons, and a velocity of 1,585 feet in a second without perforating the target ; the 32 inches of iron plating, of which this target was composed, were in reality equal to about 29 inches of solid plate, and according to the formula already referred to, the 1,700 lb. projectile, with a velocity of 1,585 feet in a second, could just completely perforate an unbacked solid plate 28 inches thick.

As plates 22 inches thick have been rolled and were actually fired at at Spezzia, there would be no difficulty in obtaining plates for Mr. Hughes's system, 14 or 16 inches thick. As we know, 14 inches is not considered beyond the thickness which can be used, and still ensure sound and well-manufactured plates. Assuming that it is, or will be, necessary to clothe our ships in armour 24 or 28 inches thick, it can be much more easily and conveniently done on the Millwall than on the sandwich system. An armour-plate 14 inches thick, backed by 12 inch hollow stringers and the other parts of Mr. Hughes's system, will give a resistance equal to that obtained by a 24 inch solid plate, which two 12 inch plates with 15 inches of teak will not do. And a 15 inch armour-plate backed by 16 inch hollow stringers would be equal to a solid armour-plate of 28 inches thick, and could only just be perforated by a 1,700 lb. pro-

jectile striking with a velocity of 1,585 feet in a second. In all these supposed cases the advantage in weight and in thickness of structure is greatly on the side of the Millwall system. The importance of the latter point must not be overlooked; for it is evident that in a turret we can preserve the interior diameter, and by diminishing the thickness of the structure we shall obtain a considerably smaller surface to cover with armour-plating.

A very few words only on the Spezzia experiments, which I take as confirming a great deal I have already said.

The thickness of armour fixed upon by the Italian Government was 22 inches of iron and 19 inches of teak, skin, frames, &c., as usual. Some of these targets were of solid armour-plates, some were sandwiches of wood and iron, and some were backed with cast iron. Most of them had been subjected to previous pounding by comparatively light projectiles; they were demolished by the 2,000 lb. projectile, 17 inches in diameter, when fired at them with velocities of 1,500 feet in a second; but in some instances, where velocities of only 1,046 feet in a second were given, the resistance to perforation was successful. As the Italian gun was far more powerful than the targets it was brought against, the gunners were enabled to give their projectiles such velocities as they pleased, and as a rule with velocities something under 1,500 feet destroyed all that was before them. No doubt the destructive effect of these large projectiles was much increased by the narrowness of the plates in proportion to the diameter of the shot. With, however, a velocity of 1,500 feet in a second, this projectile, weighing 2,000 lbs. and 17 inches in diameter, was, according to the formula already referred to, capable of just perforating completely 30 inches of iron.

The scope of this paper, and the length of time I have already trespassed on your attention, forbid me to refer to the behaviour of the steel armour-plates, and the extraordinary way in which they resisted perforation, destroyed the projectile, and fell to pieces themselves. To investigate so interesting a subject in connection also with experiments of this nature that took place at Portsmouth, and which, I understand, are even now going on, would require a separate paper, and I trust that some member of this Institution may be induced to take it up and give us the benefit of a thorough examination of all its bearings on the problem of successful defence. I have referred to these Spezzia experiments only as warnings not to be neglected of the difficulties that lie around us, and since I began this paper there has loomed before us in the immediate future, if not in the actual present, such an application of steel for guns and projectiles as will lay a still further burden on the shoulders of the defence—a burden, I confess, already almost too heavy to be borne. If steel can enable guns to bear charges of powder which will give eight-inch projectiles velocities of from two to three thousand feet in a second, and will give the projectile a power of cohesion that no opposing force can disintegrate, I own the defence can scarcely hope to prevail against the attack; but even against such projectiles and such velocities it is evidently incumbent on us to use the best system of strengthening our armour plating that the wit of man can devise.

With reference to the old and recognised powers of the attack, we have, I think, reached the limits of sandwich targets for naval construction; and one point which I omitted to notice in the slight sketch I have made of the Spezzia experiments confirms me in this opinion, that

is, the distinct indication given of the possibility of blowing off by the explosion of a well-planted shell the outer armour-plate of the sandwich. The ingenuity of a most ingenious race has undertaken to solve this problem, and perhaps it will succeed. Be that as it may, the Millwall system of armour-plating deprives these efforts of much of their importance. If I have not succeeded in convincing the gentlemen I see around me that this system should be adopted, I hardly think I can have failed to bring home to their minds that it is at least worthy of deliberate and careful trial. I understand that Mr. Hughes is in communication with the Admiralty, and I am sure the subject will receive from its Naval Constructors that intelligent and careful consideration which they are so well able to give, and which, allow me to say in passing, has borne fruit in those splendid structures of which the British Navy is composed. I trust that no naval architect, no engineer, no artillerist in this room, will believe for one moment, that in taking the course I have done, I have the presumption to suppose that I can teach them their business. Far from it, I have only endeavoured to put before them such facts as I believe to be indisputable ; it is for them to draw sound and correct inferences, to balance the possibilities, probabilities, and the difficulties that beset the solution of this great problem ; I have no interest whatever in the matter, except the all-absorbing one of procuring for our common country and for our Navy, of which we are so proud, the best possible system of defence.

No one can be more sensible than I am of the imperfections in the mode of treating such a subject which every line of this paper presents. In one respect only do I feel some little confidence in the success of my endeavours. I did not intend to say, and I believe I have not said, one word which could give the slightest pain or uneasiness to anybody, however much he may be opposed to the conclusions I have arrived at. And now, gentlemen, with many apologies for the length into which the importance of this subject has unavoidably led me, I submit this imperfect sketch for such consideration as you may think proper to give it.

APPENDIX,

GIVING DETAILS OF THE PRACTICE AGAINST THE TARGETS REFERRED TO.

BELLEROPHON TARGETS.

GENERAL DESCRIPTION.

The front consists of a solid 6-inch armour plate, behind which are 10 inches of teak laid horizontally ; between the logs of teak are four horizontal angle-iron stringers of $\frac{1}{2}$-inch iron about two feet apart, not quite reaching out to the armour. Behind this wood are two thicknesses of $\frac{3}{4}$-inch iron, forming the skin of the ship, and behind this are frames of angle-iron $10\frac{1}{2}$ inches deep by $3\frac{1}{2}$ by $\frac{1}{2}$-inch, and two angle-irons of $3\frac{1}{2}$ by $3\frac{1}{2}$ by $\frac{5}{8}$ inches riveted

together. The total area of this target was 20 feet 9 inches by 8 feet 6 inches, or 176·37 superficial feet, and weighing, without supports, 393·5 lb. per foot.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
Dec. 8th, 1863	707	Spherical Cast Iron	66½	7·92	1,380	878	35·3	Indent, 1·5 inches.
" "	708	Cylindrical Cast Iron	66·187	6·9	1,450	965	44·5	Indent, 1·2 inches.
" "	709	Cast Iron Spherical and Cylindrical	66¼	6·9	1,468	878	35·3	A Salvo, cracking and bulging the plate.
" "	710	Ditto	66¼	7.92	1,485	965	44·5	
" "	711	Ditto	66¼	6·9	1,468	878	35·3	
" "	712	Ditto	66¼	7·92	1,485	965	46·5	
" "	713	Cast Iron Cylindrical Shot.	59·9	6·0	1,524	960	44·2	Indent, 1·8 inches.
" "	714	Steel Shell	69	5·4	1,221	713	46	Indented the backing 2 inches.
" "	715	Cylindrical Steel Shell	119	7	1,273	1,336	60·6	Indented the backing 0·65 inches.
" "	716	Ditto	149·5	6·9	...	...	...	Shell burst in front of the gun.
" "	717	Cast Iron Shot	15	10·36	1,547	2,489	76·5	Cracked and indented plate to a depth of 5·2 inches; skin slightly bulged.
" "	718	Shot Steel	165	10·43	1,992	2,472	75·4	Penetrated armour, remained in backing, bulged, and cracked inner skin.
" "	719	Cylindrical Cast Iron Shot	308	10·43	1,090	2,537	77·8	Indent 1·6 inches, armour plate bent and cracked, inner skin slightly bulged.
" "	720	Cylindrical Steel Shell	149·5	6·9	1,275	1,685	78·92	Penetrated armour, burst in the backing, inner skin bulged.

The total energy of the projectiles which struck this target was 17,721 foot tons, or about 100 foot tons per foot of surface. The 6-inch armour plate was penetrated, but the structure was not.

HERCULES TARGET.

GENERAL DESCRIPTION.

The front consisted of a 9-inch solid armour-plate at the top, and an 8-inch plate below, behind which were horizontal logs of teak 12 inches thick; between these logs were horizontal stringers or edge-plates of ⅝-inch iron, the outer edge resting against the armour-plating, the inner end secured by angle-irons to the skin. Behind this cushion was the skin in two thicknesses of ¾-inch iron. Behind the skin are frames 10 inches deep, supported by two angle-irons riveted to the skin. The spaces between the frames were filled with 10 inches of teak. Behind the frames or ribs were 18 inches of teak in two thicknesses, disposed horizontally. Behind this wood a covering plate of ¾-inch iron, and behind this again angle-irons or ribs 7 inches deep. The total area of this target was 146 superficial feet, or 73 feet

for each plate. The weight for the 9-inch plate was 689 lbs., and for the 8 inch plate 652 lbs. per superficial foot.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
June 21st, 1865	1,040	Elongated Steel Shot	221·5	9·15	1,452	3,238	111·9	Struck 9-inch plate, indent 5 inches, and plate buckled over 40 inches.
" "	1,041	Ditto	299·5	10·43	1,277	3,390	103·6	Struck 8-inch plate, indent and buckle 8·51 inches over 40 inches, inside skin bulged in rear of point of impact.
" "	1,042	Ditto	221·5	9·15	1,451	3,234	111·7	Struck 9-inch plate, shot remained in plate projecting 7½ inches.
" "	1,043	Ditto	299·5	10·43	1,283	3,422	104·5	Struck 9-inch plate, grazed 8 in. plate, indent on upper plate 8 inch.
" "	1,044	Ditto	299·5	10·43	1,425	4,221	128·9	Struck on 9-inch plate, shot remained in plate, base projecting 7½ inches.
" "	1,045	Ditto	282	9·92	1,400	3,835	123	Struck on 8-inch plate, shot remained in plate, base projecting 6·5 inches.
" "	1,046	Chilled Shot	225	9·02	—	3,195	110·6	Struck on right side of hole made 1041, depth increased to 18 inches.
Dec. 7th, 1865	1,141	Steel Cylndr Shot	573	12·94	1,268	6,388	156·9	Struck 8-inch plate, shot remained in target, base of shot 4 inches within plate, inner skin slightly bulged.
" "	1,142	Ditto	574	12·94	1,76	6,480	159·4	Struck 8-inch plate, small crack right through plate.
" "	1,143	Chilled do.	577	12·95	1,310	6,872	169	Struck top of 9-inch plate, ploughed through all the backing to a depth of 42 inches.
" "	1,144	Ditto	577	12·95	1,310	6,872	169	Penetrated the target; struck 8-inch plate where previous shot had been.
" "	1,145	Ditto	580	12·95	Not observed	6,872	169	Struck 8-inch plate, shot broke, depth of indent 1 foot 10 inches.
" "	1,146	Steel Clndr. Shell	567	12·94	1,307	6,482	159.5	Struck 9-inch plate, cracked it through, shell broke up.

The total energy of the projectiles which struck the target was 64,499 tons, or 442 tons per superficial foot, divided as follows:—The 9-inch plate received about 378 tons per superficial foot; the 8-inch plate received about 513 tons per superficial foot; the target was penetrated here, but not in a sound part. The report of the Committee was that it could not be penetrated in a sound place by any guns then existing.

MILLWALL TARGET.

GENERAL DESCRIPTION.

The front consisted of two iron plates, one 6 inches, the other 9 inches thick, with a port-hole cut equally out of each. Part of the 6-inch plate is covered by three 1-inch plates, covering more than half the length of the plate and the whole of its depth. The two principal plates are 12 feet 2 inches long and 4 feet wide. Behind these plates are a series of hollow stringers 7 inches deep, running horizontally, their outer faces in contact with the inner part of the armour-plates. The inner part of the stringer has two flanges riveted to a skin formed

of two thicknesses of $\frac{3}{4}$-inch iron. Behind this there is a vertical hollow stringer of the same description, the hollow part turned outwards. Both stringers, where hollow, are filled with wood. At the back of these vertical stringers or framers there is a covering plate or inner skin of 1-inch iron. Various strengthening pieces were added in the way of the port-hole. The total area, including the port-hole, was 97 superficial feet. The weight per foot of the 6-inch plating was 558 lbs., of the 9-inch 678 lbs.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
July 16th, 1868	1,581	Palliser Shot	249	8·92	1,254	2,715	96·9	Struck 9-inch plate; penetration, 10 inches.
,, ,,	1,582	Ditto	249·5	8·92	Not observed	2,715	96·9	Struck 6-inch plate; penetration, 14·3 inches.
,, ,,	1,583	Ditto	250	8·92	1,251	2,713	96·8	Struck 6-inch plate faced three 1-inch plates; penetration, 12.8 inches.
,, ,,	1,584	Ditto	250	8·92	1,234	2,650	94·6	Struck 9-inch plate; penetration, 9·9; shell rebounded.
,, ,,	1,586	Palliser Shell	250·75	8·92	1,310	2,984	106·5	Struck 9-inch plate; indent, 12 inches; head of shell flew back.
,, ,,	1,587	Ditto	248·6	8·92	Not observed	2,984	106·5	Struck 6-inch plate; penetration, 10 inches; head of shell remained in target.
,, ,,	1,588	Ditto	248·6	8·92	Shell	burst	in gun	Fragments indenting the target.
,, ,,	1,590	Palliser Shot	251	8·92	Not observed	2,985	106·5	Struck the 6-inch, faced with 3-inch; damaged the stringers considerably; shot-hole in contact with 1,583; penetration, 24·8 inches.
,, ,,	1,591	Palliser Shell	399	9·92	,,	4,027	129·2	Struck bottom of 9-inch plate; passed under shield, burst, and made a large hole.
,, ,,	1,592	Ditto	397·75	9·92	,,	4,519	145	Struck junction 9 and 6-inch plates on the indent of 1,586; penetration, 19 inches.
,, ,,	1,594	Ditto	399·2	9·92	,,	4,519	145	Struck 9-inch plate; penetrated 22 inches; projectiles, 27 3 inches long.
,, ,,	1,595	Spherical Cast Iron Shot.	451	14·89	1,156	4,182	89·4	Struck 9-inch plate, shot rebounded 6 feet, partly broken, and set up.
,, ,,	1,596	Ditto	455	14·89	1,399	6,542	139·9	Struck top corner of port, 6-inch faced, 3-inch plates partly in the port; shot broke up.
,, ,,	1,593	Ditto	397·7	9·92	Shell	burst	in gun	Head struck in a splash on 9-inch plate; indent, 4 inches.
,, ,,	1,589	Palliser Shell	248·6	8·92	Not observed	2,715	969	Struck the junction of 6 and 9-inch plates; indent, 12 inches.
Sept. 22nd ,,	1,596	Palliser Shot	601	11·72	1,171	5,714.5	1,526	Struck 6-inch plate close to the port, where the target was only 18 inches thick, passed through, and broke up.
,, ,,	1,597	Ditto	596	11·92	1,154	5,503.8	146·97	Struck 9-inch plate; penetration, 15·1 inches.
,, ,,	1,598	Ditto	600	11·92	1,171	5,705	152·35	Struck 6-inch plate; indent touches 1,587; indent, 33 inches.
,, ,,	1,599	Ditto	596	11·92	1,181	5,764	153 93	Struck 9-inch plate; indent, 23·2 inches.

The area of the 9-inch plate, including the port-hole, was 48 ft. 6 in. The total energy of shot it received was about 35·960 foot tons, or nearly 740 foot tons per superficial foot. The area of the 6-inch plate unfaced was about 24 feet, and the total energy of shot it received was about 18,476, or about 760 foot tons per superficial foot; and the area of the faced part of the 6-inch plate was about 25 feet. It received a total energy of projectiles of 11,598 foot tons, or 465 per superficial foot. Of course, if the port-hole were deducted the areas would be smaller and the energies per superficial foot larger.

The bolts in this target were of peculiar construction, and stood remarkably well.

No. 33 TARGET, THICK PART—SANDWICH.

GENERAL DESCRIPTION.

The thick portion of this target consisted, in front, of an 8-inch solid armour-plate, behind which were 8 inches of teak, then a 5-inch iron plate, then 6 inches of teak, horizontal stringer plates, a skin $1\frac{1}{2}$-inch thick, with frames $12\frac{3}{4}$ inches apart. No dimensions of this target are given. It appears to have received projectiles the total energy of which was 25,687 foot tons. Its approximate weight was about 709 lbs. per superficial foot.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
July, 1871	1,836	Palliser, Large Core	214·7	8·92	1,310	2,912	103·9	Total penetration 18·2 inches, head of shot remaining in indent.
" "	1,837	Ditto	245	8·92	Not	obser	ved	Total penetration 17·9 inches, head of shot remained in indent.
July 12th, 1871	1,830	Ditto	530	10·92	1,250	5,742	167·4	Passed through—almost in a line with rib, which it tore from the skin and distorted.
" "	1,832	Ditto	533·4	10·92	1,275	6,013	175·3	Burst and passed through, setting the backing on fire; frames broken and distorted.
July 21st, 1871	1,842	Palliser Shell	527	10·92	1,258	5,783	168·6	Passed through, bursting when passing through inner skin.
" "	1,843	Palliser, Large Core	539	10·92	1,184	5,239	152·7	Passed through, burst apparently on passing through the skin.

The 11-inch gun, weighing 25 tons, was thus completely master of this sandwich target, though its total thickness was, iron $14\frac{1}{2}$ inches, and wood 13 inches.

No. 34 TARGET.

GENERAL DESCRIPTION.

The outer portion consists of two 14-inch solid armour plates, one above the other, 14 feet 8 inches long by 3 feet 8 inches deep, backed by 9-inch vertical and 6-inch horizontal oak logs. Skin two thicknesses of $\frac{5}{8}$-inch iron, horizontal stringers, frames, and covering plate $\frac{1}{4}$-inch thick behind all, the whole bent to a radius of 15 feet $7\frac{1}{2}$ inches to represent a turret. The area of the target was 102·5 superficial feet. The approximate weight per superficial foot was 726 lbs. in its thickest part.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
May 4th, 1871	1,792	Palliser Shot	593	11·92	1,269	6,622	176·8	Penetration, 14·3 inches; considerable disintegration on rear; many fragments picked up.
,, ,,	1,794	Do. Shell	601	11·92	1,258	6,595	176·1	Struck on a bolt; penetrated 10·75 inches; shell burst and broke on striking; many fragments picked up in the rear.
May 5th, 1871	1,803	Do. Shot	534	10·92	1,292	6,181	180·2	Penetrated 24 inches (projectile, 28·5 inches); much disintegration; and many fragments picked up in the rear.
,, ,,	1,804	Ditto	534	10·92	1,289	6,152	179·3	Struck a bolt, which it drove clean through; penetrated 19 inches; shot rebounded; many fragments picked up in the rear.
May 7th, 1871	1,808	Ditto	534	10·92	1,305	6,259	182·4	A piece of top plate broken away, 20·5 by 11 inches; struck the edge of the plate; some fragments picked up.
,, ,,	1,809	Ditto	533	10·92	1,303	6,275	182·9	Target thrown down; shot passed through, plate and backing broke up, and passed in; fragments over inner skin, breaking it in.
July 6th, 1871	1,822	Palliser, Large Core	537	10·92	1,263	5,940	173·1	Target re-erected, shored up; penetration, 13·5 inches; plate cracked; damage to the rear.
,, ,,	1,823	Palliser Shell	542	10·92	1,265	6,014	175·3	Penetration, 16·75 inches; much damage in the rear,
,, ,,	1,826	Ditto	599·5	11·92	1,222	6,208	165·8	Not a direct hit; but the point of the shell penetrated to the backing; Target obliged to be shored.

The report notices that the target was very badly supported; it states that, allowing for such rounds as merely grazed the target, the total energy of the projectiles which struck direct was 53,117 foot tons, or 518 foot tons per superficial foot of the structure, and though just able to resist the guns brought against it, afforded but little margin of protection.

The inner skin was not better preserved than in No. 35, though the depth of penetration into No. 34 was less than in the former.

No. 35 TARGET.

GENERAL DESCRIPTION.

It is in all respects similar to No. 34, with the important exception of the armour being divided into two thicknesses, 8 and 6 inches, of iron, with 9-inch oak behind the 8-inch plate, and 6-inch oak behind the 6-inch plate. Frames and other things as in No. 34. The area of the target was about 102·5 superficial feet. It was bent to the same radius as No. 34. Its approximate weight per superficial foot was 726 lbs. in its thickest part.

Date of Experiment.	Photographic Number of Shot.	Projectile. Nature.	Projectile. Weight.	Projectile. Diameter.	Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
			lbs.	inches.	feet.	ft. tons.	foot tons.	
May 4th and 5th, 1871	1,792	Palliser Shot	595	11·92	1,258	6,529	174·4	Struck on junction of plates, penetration 27 inches, many fragments in rear.
" "	1,795	Do. Shell	603·8	11·92	1,254	6,584	175·8	Depth of indent in armour 10 inches; a glancing shot.
" "	1,799	Do. do.	592	11·92	Missed	and	glanced	No result.
" "	1,800	Do. Shot	594	11·92	1,262	6,560	175·2	Shot penetrated its whole length 26·2 inches into target, the base 0·6 inch inside the face, outer plate cracked through damage in rear.
" "	1,802	Do. do.	536	10·92	1,291	6,194	180·6	Total penetration 24 inches, length of shot 28·5, cracked plate.
" "	1,810	Do. Shell	538	10·92	1,306	6,363	185·5	Penetration 28 inches, projectile 28·5; much damage in the rear, many fragments picked up.
" "	1,812	Do. Shot	398	9·92	1,325	4,845	155·5	Penetration 22·5 inches, projectile 24·5 inches; much disintegration and damage.
July 6th, 1871	1,819	Do. Shell	601	11·92	Not	recorded.		Glanced, and scooped the target slightly.
" "	1,820	Do. do.	599	11·92	1,218	6,164	164·6	Total penetration 23·5 inches, projectile 26·2 inches; cracks and damage done.
" "	1,824	Do. do.	593	11·92	1,227	6,191	165·3	Struck on top edge close to 1,799, and burst on top covering plate.
" "	1,825	Do. do.	592·4	11·92	1,235	6,265	167·3	An indirect glancing hit, not reaching the inner plate.
" "	1,821	Do. large core	531	10·92	1,263	5,940	173·1	No hit.

This target was also badly supported, much disintegrated by the firing, many fragments thrown to the rear, as in No. 34. Allowing for the direct hits only, it received a total energy of projectiles amounting to 51,656 foot tons, or about 503 foot tons per superficial foot. Penetration into this target was certainly greater than into No. 34, but the inner skin was at least equally well preserved.

"GLATTON" TURRET IN PLACE.

GENERAL DESCRIPTION OF TARGET AT PLACE FIRED AT.

Outside a solid 14-inch plate; next to which three vertical logs of teak 7 inches thick; next to which horizontal logs of teak 5 inches thick, with horizontal edge strips or stringers between them, riveted by angle-irons to the skin, which is on two thicknesses of $\frac{5}{8}$-inch iron frames; behind, $9\frac{3}{4}$ inches deep, secured with two angle-irons to the back part of the skin, and a covering plate or lining behind all of $\frac{1}{4}$-inch iron. A square foot of that part of the target subjected to the experiment weighed about 726 lbs.

Date of Experiment.	Photographic Number of Shot.	Projectile. Nature.	Projectile. Weight.	Projectile. Diameter.	Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
			lbs.	inches.	feet.	ft. tons.	foot tons.	
July 4th, 1872	—	Palliser Chilled Shot	600	11·92	1,275	6,750	181	Penetration 23·2 inches, length of projectile 26·2 inches; shot broke up, half of it remaining in the structure.
" "	—	Ditto	600	11·92	1,275	6,750	181	Grazed the glacis plate, broke the covering plate of the deck, penetrated $12\frac{1}{2}$ inches.

No material damage was done. The turret revolved after receiving these shots. The second was a very oblique blow, and the distance from centre to centre of the projectiles was

about 9 feet 8 inches. The live creatures inside the turret were not hurt, and the guns were run in and out without difficulty; but the projectile very nearly penetrated the structure completely, the skin being split over the point of the shot, and wood pushed through the split. One rib was broken.

No. 40.—A SANDWICH TARGET.

GENERAL DESCRIPTION.

This target consisted of three armour-plates 6½ inches thick, one behind the other. Behind the first was a layer of 5 inches of teak, then the second armour-plate and 5 inches of teak (the layer being horizontal and vertical); then the third armour-plate; then the skin, ribs, &c. In the first experiment the weight of the target was about 907 lbs. per superficial foot; in the second experiment, 6½ inches of iron plate and 5 inches of teak having been added, its approximate weight was 1,215 per foot, tried against the 38-ton gun.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
October, 1876	2,039	Palliser Shell	815	12·4	1,421	11,408	290	Passed through, setting the backing on fire; a portion of the base of the shot remained in the target.
			Second experiment, gun chambered.					
March 10th, 1877	2,043	Palliser Shell, 33 in. long	812	12·4	1,525	13,045	335	The penetration was about 20 inches in all, the base of the shot getting just within the first armour-plate, and the point just penetrating the inner plate; the length of the projectile was 33 inches.

The Committee report, on the first experiment, that the gun and the target were about equal; on the second, that the resisting power of the target was the greatest.

No. 41.—A SANDWICH TARGET.

GENERAL DESCRIPTION.

This target consisted of four 8-inch solid plates, disposed as follows:—Outside of all, an 8-inch armour plate, 16 feet by 10 inches, behind which is a layer of teak 5 inches thick, horizontally and vertically. Three such plates, with intervals of similar fillings of teak, follow. There does not appear to have been any skin or representation of a ship's frames, &c. The total weight per superficial foot was, approximately, 1460.

Date of Experiment.	Photographic Number of Shot.	Projectile.			Velocity on Impact per Second.	Total Energy of Projectile on Impact.	Energy per inch of Shot's Circumference.	General Remarks.
		Nature.	Weight.	Diameter.				
			lbs.	inches.	feet.	ft. tons.	foot tons.	
Feb. 1st, 1877	2,041	Palliser Shell, 42 in. long	1700	15·87	1,496	26,400	526	No complete penetration was effected; depth of indent, 48·8 in.
			Second experiment, gun chambered.					
May 4th, 1877	2,047	Ditto	1700	15·92	1,585	26,907	2.590	No complete penetration was effected; depth of indent, 56 in.

The target being too powerful for the gun, the penetration was much less than if the power of the projectile and resistance of the target had been more nearly equal.

ARMOUR FOR SHIPS.

By N. Barnaby, C.B. (Vice-President I.N.A.)

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

Having received an intimation from one of our Vice-Presidents, Vice-Admiral Sir R. Spencer Robinson, that he proposed to question the propriety of the existing mode of applying the armour to English ships, I thought I might best facilitate profitable discussion upon his paper by stating my own views as to the present position of the question of the material for ships' armour, and the best mode of applying it to the parts to be defended. Before proceeding to do so I would remind you of a few historical facts, and make some remarks of a general character relating to armour for ships.

The modern ironclad ship owes its origin to an imperial order to the French Minister of Marine, dated from St. Cloud, 16th November, 1854. The Emperor Napoleon III. pointed out that in warfare there must be, in addition to courage and ability, even chances. Actions on the land are avoided so long as there is no chance of success. So in naval warfare, if the fleet were risked it would be in the hope of destroying that of the enemy. An enormous capital is hazarded in order to destroy that which has cost the enemy as much. But, he went on to say, so soon as the fleet is employed in the attack of a fortification the proportions are entirely altered, for not only will a ship be found inferior to a land battery, because a ship offers a large object to strike, while the land battery occupies but a little space, and is protected by parapets, but also the stake is materially different. So it happened that in the Black Sea 25,000 sailors and 3,000 guns could not seriously injure the Russian fortifications; and that indecisive attacks were made at other places, entailing serious damage to the ships without doing any material harm to the enemy. Under existing conditions risks were incurred for nothing. If the ships threw their projectiles at 2,000 metres they consumed their ammunition at a dead loss, and gave a false idea of the power of the fleet. If they approached nearer they exposed the State to sacrifices too considerable in proportion to the object in view, for it would generally be perfectly senseless to risk the loss of a fleet for the destruction of a few forts. To remove this difficulty he proposed to create "*une flotte de siége*," capable of producing decisive effects, and at the same time of lessening to the State the chances of loss in men and money.

The course taken to meet the Emperor's views was to build floating batteries with

numerous light broadside guns, and covered from end to end with iron armour-plates. This met the necessity for the moment, but when the French Government in 1860 took a further step and launched a sea-going ship, the *Gloire*, covered with the armour of the floating batteries, they placed us upon the road to our present position, which is more exposed than ever to the Emperor's objections of 1854. Fresh means of attack were organised, by which the armour could be penetrated, or by which the unarmoured parts could be reached. So it happens that in 1879 the ships are far more costly and carry fewer guns than in 1854, and are at the same time exposed to destruction by the torpedo defences of the port as well as by its guns, while the *flotte de siége* of 1854 is useless against the guns of the ports, and defenceless against their torpedoes. It would, perhaps, have saved the expenditure of millions of money, if the Council of Ministers who met to consider the Emperor's message had proposed a *flotte de siége* consisting of small unarmoured gunboats of eight or ten knots' speed, mounting guns of greater range and power, instead of proposing to mount the existing guns in protected ships. Such a siege fleet would have met the necessities of to-day as well as those of 1854.

As to the sea-going fleets, we have already proceeded far in stripping the armour from their sides, and coming back to the old condition, but with this important difference: the vital parts of the fighting machine are protected against the enemy's guns to such an extent that no single shot or shell shall be capable of disabling it. This protection, however afforded, appears to be indispensable in ships designed under such conditions of warfare as now exist. This is the nature of the protection given to the great Italian first-class battle-ships *Italia* and *Lepanto*. They are to have enormous power of engines and guns, but they are not *ironclads*. They are *protected* ships, and it cannot be denied that they are also *armoured* ships. They have an underwater deck of three-inch armour, weighing about 1,200 tons, and they have probably an equal weight of armour of eighteen to twenty-seven inches protecting their internal vital parts.

Not less interesting is the protection which has been recently devised in England for the merchant ships employed in a time of war under the Queen's flag. No amount of resistance which an armour-plate could show, would give the satisfaction which I received from the excellent behaviour of the simple combination of fuel, and thin loose iron sheets. (See Addenda.)

Coal armour and torpedoes together have given to the fast merchant shipping of England a significance in warfare wholly new. In the place of being helpless wards to be defended, they become active combatants—a strength instead of an embarrassment.

I have thus gone lightly over the ground covered by the history of armour from 1854 to 1879. I have shown that in its latest stages it consists on the one hand of plates, more than two feet thick, and on the other it is the ship's own fuel, and that in both it is employed only to protect the absolutely vital parts. For this purpose it appears to be indispensable, and I suppose it will become general, and will be permanent.

The material of which thick armour should be made is now the subject of anxious experiment in Germany and France, as well as in Italy and England. Steel and other alloys

of iron are so little known or understood, that there is a large field for experiment open for the armour-plate maker and for the artillerist.

Steel has been tried many times alone, and in combination with iron, but it never gave enough satisfaction to secure its adoption until experiments were made at Spezzia with plate 55 centimetres thick, manufactured by Schneider et Cie. at Creuzot. With these the Italian Government were so well pleased that they are plating the *Dandolo* and *Duilio* with such plates. Those of us who visited the French Exhibition last year may have seen a steel armour-plate produced by this firm, bent to the form of a turret, 32 inches thick, and weighing 65 tons.

Those who went to the works at Creuzot, by favour of Mons. Schneider, may also have seen an ingot of cast steel, suitable for making an armour-plate, and weighing 120 tons. The rival firm of Terre Noire exhibited armour-plates of steel which had not been hammered, or rolled, or otherwise forged. They were simply cast plates, tempered in oil, and annealed. Judging from the admirable series of specimens and tests and analyses, the manufacture has already obtained a large degree of precision, and is full of promise.

I believe that the Italian Government will shortly test some of it with the big Elswick gun, and it may be that blocks of cast steel will revolutionise the manufacture of armour by making the rolling operations unnecessary, and bringing down the cost to that of ordinary large castings. The French Government have also made many experiments with steel armour at Gâvre, but have not yet satisfied themselves that they should give up wrought iron.

Experiments with steel in England have shown that steel can be made of great hardness, so hard that it will break up all projectiles which strike it, and that will not suffer seriously in doing so. Sir Joseph Whitworth has obtained some most remarkable results in this direction, and he is still pursuing the inquiry. (See Addenda.)

The Sheffield makers, Messrs. Cammell and Messrs. Brown, have both declared in favour of iron plates with steel faces, produced in the rolls. Both these firms exhibited at Paris last summer the comparative results of a shot at each of two plates rather less than four feet square, and 9 inches thick. In all the plates a 7-inch gun was used at 70 feet range, and a Palliser chilled shot. The iron plate made by Brown was indented 8 inches without cracking, with a charge of 25 lbs. of powder. That made by Cammell was perforated, without cracking, with a charge of 30 lbs. of powder.

The Browns' compound plate was half steel and half iron. With a charge of 30 lbs. of powder it was only indented $4\frac{3}{8}$ inches, and there was no cracking.

The Cammells' compound plate had 5 inches of steel and 4 inches of iron. With a charge of 30 lbs. of powder it was only indented 3·12 inches, but the steel part of the plate opened from the hole to one edge $\frac{1}{8}$ of an inch. The crack did not go through the iron. The Jurors awarded a Grand Prix to both exhibitors, and the honour was well deserved.

At Shoebury a blow which has perforated a 12-inch iron plate has been delivered three times at a 10-inch compound plate 6 feet square, without any one of the three getting through the plate. (See also Addenda.)

The conditions laid down for the turret plates of the *Inflexible* are, that a piece about

6 feet long, cut off the bent plate of the turret, and secured to backing, shall neither be perforated nor cracked through by the shot which would perforate a similar piece of iron ; and that if the edges of the test piece are supported by a frame, no one of three such shots planted at 2 feet intervals shall get through.

The construction of the turret wall differs in two important respects from that which Sir Spencer Robinson favours.

1. It has considerable depth. The armour is 16 in. thick, and the backing is 18 in. thick.

2. The armour is in two thicknesses instead of one.

1. There is a distinct advantage in depth of target. This has been very well put by a celebrated steel maker, when proposing a particular form of target. He said : "The force residing in a projectile may be estimated by the average powder pressure in the gun, multiplied by the distance through which it has been active."

"Assuming the effective powder pressure to be equal to $7\frac{1}{2}$ tons on the square inch, active through a distance of 5 ft. against a projectile of 9 in. diameter, weighing $2\frac{1}{2}$ cwt., we have a total force of $64 \times 5 \times 7\frac{1}{2} = 2{,}400$ foot tons, represented by an initial velocity of 1,100 ft. per second. Assuming the projectile to be a hard substance, and that its motion is to be arrested by hard armour, this amount of force would have to be consumed within an indefinitely short space of time, during which the force of impact exerted would be infinite in amount. In other words, either the projectile or the armour it strikes, or both of them, must be broken up, whilst, if the target struck by the projectile possess a certain amount of ductility or penetrability, the former will be penetrated until the force residing in the projectile be absorbed."

So that if the target offers a certain amount of resistance continuously, after perforation is commenced, and before the skin of the ship is reached, the energy is taken out of the projectile by a resistance comparable in amount with the original powder pressure. The target may be of such depth as to need only a continuous resistance, equal to or even less than this original pressure.

To this we may add, that to receive a projectile made of comparatively brittle material, as chilled cast iron, it is desirable to make the outer surface of the armour as hard as possible, so as to resist the penetration of the point or front of the projectile, and thus to cause it to break up. This it often does, and simply plasters itself upon the face of the plate. Moreover should it get through the front plate, a space of comparatively soft material behind allows the projectile to break up and disperse in this space, as it will in an air space between two armour-plates. This effect may be often seen when a Palliser projectile passes through one plate and then through wood backing before it reaches the inner plate.

2. The arrangement of the armour in two thicknesses, rather than in one, results from a set of trials made at Shoebury in 1871 on the turret targets Nos. 34 and 35.

No. 34 target consisted of two solid wrought-iron armour-plates (upper and lower), each 14 ft. by 3 ft. 8 in. by 14 in., on 15 in. of timber backing, and one of 6 in. No. 35 target consisted of two similar plates, 8 in. thick in front, and two of 6 in. thick within. The inner thickness was interposed between the two thicknesses of backing. The

total depth of target in each case was 30¼ in., and the targets were bent to a radius of 15 ft. 7½ in.

They were fired at by the 12-inch and 11-inch 25-ton guns, with 600 lb. and 530 lb. projectiles, and 85 lbs. of pebble powder, at 200 yards range. The two targets were struck by projectiles representing over 500 foot tons per square foot of surface. The committee, consisting of General Elwyn, Captains Foley and Arthur, of the Royal Navy, Col. Inglis, R.E., Captains Harrison and W. H. Noble, of the Royal Artillery, and Lieut. Innes, R.E., decided that both targets were a match for the guns, projectiles, and charges with which they were attacked, although they afforded but little margin of security.

That although the actual amount of indent, as measured from the face of the target, was less in No. 34, the Committee considered that the absolute amount of resistance to shot and shell—that is to say, the protection afforded to the inner skin—had been shown to be practically the same in both targets.

That the importance of diminishing the number of joints in the armour of turrets was clearly demonstrated during these experiments. Turrets constructed on the model of either No. 34 or No. 35 targets would present a centre line of weakness all round the junction of the upper and lower plates.

That although the manufacture of a 14-inch plate of width sufficient for the height of the turret might be impracticable with existing appliances, plates of sufficient width, up to 8 in. thick, could be made, and the Committee on that account considered that No. 35 target, as designed and constructed, did not fairly exhibit all the merits of the double-plate system. The advantages to be obtained by the use of two or more layers of thinner plates of greater width were that the number of joints and through fastenings might be diminished, the necessity for through joints altogether avoided, and that the construction about the ports might be greatly improved.

Taking therefore into consideration that the double plate system admitted of the employment in turrets of armour-plates of a breadth equal to the height of the turret; that through joints could thus be dispensed with; that the bolting and securing of the structure was facilitated; that the absolute cost was less;* and that the total resistance did not appear to be practically affected; the Committee preferred this construction to that in which the armour was disposed in one thickness.

After the trials of the double and single plate systems at Spezzia, the War Office was again referred to on this point, with special reference to the possible effect of gun-cotton shells bursting between the two thicknesses, and it advised adherence to the double plate plan. I shall, however, be quite prepared to find, at no distant date, that the progress in steel manufacture, affecting both armour and projectiles, requires us to put the whole of our defensive plates into one thickness. Up to this point I believe our construction is a good one.

* An error, pointed out at the time.

ADDENDA.

Cammell's Compressed or Steel-faced Plate.

A plate 9 in. thick and 8 ft. by 6 ft. 9 in. in length and breadth was made of 5½ in. of iron behind 3½ in. of steel, the two metals being thoroughly welded together by Mr. A. Wilson's patent process. It was secured to wood backing on board the *Nettle* at Portsmouth by thirteen 3½ in. bolts, screwed in from the back. It was fired at by the gun loaded in the same manner as when it easily perforated a 9 in. iron plate similarly backed. The gun was the 9 in. M. L. R. gun, with a 250 lb. chilled cast-iron projectile and 50 lbs. of pebble powder. The range was 30 ft.

This plate received five successive blows from this gun without perforation. At the first blow there were some very fine cracks of the steel face. At the second shot there was a through crack. The point of the shot penetrated 7·37 in., and both holes remained plugged up by the points of the projectiles. The third shot did not penetrate so deeply as either of the others, but there was some extension of the fine cracking. The fourth plate only penetrated 6½ in , but drove out an armour bolt and cracked the plate right across. The fifth shot penetrated 8 in., and broke off the lower corner of the plate, exposing the backing, and therefore breaking down the defence.

The final result was that this small plate kept out five projectiles, every one of which would have gone through the same thickness of iron, and would apparently have been capable of receiving several others had it been supported round the edges by adjacent plates.

Sir Joseph Whitworth's Steel Armour.

I.

A small 5 in. plate of steel 2 ft. 3 in. square, having hard steel bolts (2½ in. diameter) screwed into it at intervals of 3½ in., was fired at from a Whitworth 32-pounder gun. Each projectile weighed about 32 lbs., and the charge was 6½ lbs. of R.L.G. powder.

The first shot was of chilled cast iron. It penetrated only 2·37 in.

The second shot was of the same material. It penetrated 1·4 in.

The third shot was of steel, and it penetrated 3·2 in.

The plate is at the Admiralty, and there is not the least indication of cracking in it. The steel shell remained whole, but was shortened 2·2 in., and bulged. The base of the shell was not altered in form, but at 4·5 in. from the base, the diameter of the shell across the corners was increased 0·6 in.

II.

A circular shield, about 5 ft. across and 9 in. thick, made of concentric screwed rings of steel, fastened by a central screw to a flat plate of iron, received a blow from a Woolwich chilled iron 9 in. projectile planed to fit a Whitworth 9 in. gun.

The projectile weighed 250 lbs., and was fired with 50 lbs. of powder.

The projectile should have gone through the shield had the resisting power been the same as iron, but the actual penetration was only 1·4 in.

DISCUSSION ON THE TWO PRECEDING PAPERS.

Mr. Chas. F. Henwood: As I have had a great deal of experience with Hughes' Stringer for the last thirteen or fourteen years, I may perhaps be allowed to make a few remarks. In 1866, or earlier than that, Mr. Hughes first introduced his backing, and I had in the course of my profession to elect the most suitable backing for some ships I was then designing for the late Admiral Halsted, and in conjunction with Mr. Watts, after examining all the targets and experiments that had taken place at Shoeburyness up to that time, we came to the conclusion that nothing better had been presented for a backing for armour-plating than this invention of Mr. Hughes; and have confirmed our judgment from the subsequent results of experiments (and I have watched them very carefully as they have transpired). Sir Spencer Robinson, in his paper, institutes a comparison between the *Devastation* side or turret and its equivalent on the Hughes' system. He says there would be a gain of some 48 tons in an area of 150 feet, by 15 feet; but 150 feet is rather a short length. If you take a vessel 300 feet long, the saving in weight of defensive armour, other things being equal, will then represent some 200 tons. But that is not all the saving, in addition to equal efficiency, because to carry these 200 tons of armour we have more or less to provide 200 tons of hull of the ship, so we not only save those 200 tons of armour, which may be taken at £60 a ton, but we have 200 tons off the hull of the ship, which would probably cost some £60 or £80 a ton. So that in the case of one ship there is a saving alone in money, maintaining an equal efficiency, of some £24,000, and that extending over, say twenty ships, represents pretty nearly half a million of money. But there are other advantages in that you may, with equal defensive powers, obtain a very much smaller ship, and consequently a handier one, with less draught of water. All these ought to be borne in mind in considering this question, because any improvement that can be made, whereby we can obtain a reduction in the weight to be carried, is not merely represented by the saving in the weight, or in the thing itself. I was not in the room during the reading of most of the paper, and I content myself with just these few remarks in support of what Admiral Robinson has so well stated in his paper.

Mr. N. Barnaby: My Lord, I may perhaps help the discussion a little if I refer to one or two facts to which I have asked Sir Spencer Robinson's attention, namely, that there is a difference in the statements given between the weight of the Millwall shield per square foot in Sir Spencer Robinson's paper, he having taken a certain view of it, and the weight which is assigned to it by the official people who were employed at Shoeburyness in connection with the experiments. I hold in my hand a paper belonging to the War Office, which was lent me when I made inquiry of them as to what were the facts as to its weight, and I will just read, with your permission, a word or two from that statement of theirs. The officer who is directly responsible for this statement is Colonel Inglis, so well known to us all, who is at the head of the Fortification Branch of the War Office. He says that the shield, before it was fired at, was very strongly supported at the back, and that the Committee who directed the firing experiments saw that the shield had a very large resisting power; but they did not take into account what was the weight which was required in order to give the defence which they saw to be so great. They did not contemplate that such a shield would be put forward, or at any rate that they were called upon to give any opinion as to its suitability for a ship; but when these gentlemen from the War Office, Colonel Inglis' department, came to weigh it, they said this, that the total weight of the shield was so increased that the lower portion of what is called the 9-inch plate was equivalent to $23\frac{3}{10}$ inches of solid iron: in other words, instead of having a weight of, I think, 678 lbs. for the 9-inch plate, these officers make out that it was 952 lbs.; and instead of being 558 lbs. per square foot for the 6-inch plate, they say it was not so at all, but it was 824 lbs. Now the difference of statement with regard to the weights is so serious, if you propose to use it in a ship, that

I thought it my duty at once, as soon as I knew it, to call Sir Spencer Robinson's attention to it; because we must all be conscious how extremely anxious he has been to put the case carefully. You will see in his paper he only makes a very small gain in weight as between the 9-inch structure which he is dealing with, and the *Devastation* armour which he contrasts with it. He does a little injustice to the *Devastation* armour in his statement, because the target which was fired at represented the *Devastation* turret, and that only had fifteen inches of backing, and an inch and a quarter of skin; whereas he took eighteen inches of backing, and an inch and a half of skin. He takes a part of the side wall of the ship that was not fired at, and attaches that weight to the turret wall that was fired at. The weight he should have taken is 726 lbs. and not 751 lbs.* There is another fact to which I want to call your attention, which is this. The War Office people were so desirous of ascertaining what was the precise value of Mr. Hughes' bars that they had a target expressly devised, for the purpose of testing the comparative resisting power of the Hughes' bars, of some channel irons placed edgewise, and of solid plates; and the report in this book which I hold in my hand is this, that 8-inch solid plates on a 2-inch skin and concrete backing—that was, the skin and backing were common to all the plates—offered better resistance to projectiles of all calibres than the same quantity of iron arranged partly as plates, and partly as stringers in rear of the plates, supported by a similar backing; that of the two systems of stringers experimented upon—that is, Mr. Hughes' system, and the double channel iron placed edgewise, neither form exhibited any decided relative advantage; and giving in general terms the result of the experiment, they say that the compound construction of plate and stringer offers no advantage, as regards penetration, over the solid plate upon plate system. Now, Sir Spencer Robinson is not taken by surprise in what I have just said, and, as he has just told me, he is prepared to say a word or two on it; but I thought it best that I should at once call your attention to what seems to me to be a very serious matter, namely, Are the statements put before us this morning as to the gain in resisting power depending on relative weights to be accepted? In my judgment they are not to be accepted.

Mr. Mackie: My Lord, as I was present at the shooting at the Millwall shield, which, if I remember rightly, took place at the time you were in office, and in which you took on one occasion very great interest—the occasion to which Mr. Barnaby refers—I should like to say a few words on behalf of the Millwall shield, particularly as Mr. Barnaby has made reference to one matter on which I should have thought he would have had no doubt in his own mind, that is, the question of weights. The Millwall shield was an experimental structure, made by Mr. Hughes at Millwall, to the order of the War Office, and was designed and made as a land shield. I need not point out to you, my Lord, nor to this audience, that the question of weight is of no moment at all in the defence of a land embrasure, because the weight can be carried on the ground in any ordinary manner, and because also it is the cost of the defence there which is the main object; and if Mr. Hughes could produce on that occasion a shield which weighed 38 tons, as against 28 tons in the shield which was its competitor, and produce that better and stronger shield for the same sum of £1,000, then what I say is, that in that direction he was right on that occasion. But if you are to put the same qualification of armour defence on a ship, it must be quite apparent to anybody's mind that you must carry your defence on the smallest weight you can, and in such a case as that, you have no right to estimate the floor plate, the backing, and the arrangement of it altogether, which amounted in this case to a very serious item, I think not much less than 12 or 14 tons. You add that to the weight of the shield on land, but not to Hughes' system at sea, because you make the ship a part of the structure. I do not know whether Mr. Hughes has the model of his original turret here, but here is one that will answer my purpose. This is the face plate of the shield shot at on that occasion. Here is a model of the shield ordered

* This correction was made by Sir Spencer Robinson before his paper was printed.

by the Royal Engineers, with one portion covered by an armour-plate of 6 inches: another portion by a 9-inch plate: and a third portion in which part of the 6-inch plate is covered by three thicknesses of 1-inch plate. Mr. Hughes' proposition which he sent in a design for in 1865, three years before the Royal Engineers' shield was known or fired at—three years before his own shield was fired at—that shield which he sent in the original contract for, had a uniform face plate of 8 inches, and a hollow stringer backing of 7 inches. I need not stop to point out the superior value of that design over an experimental shield of this kind. If you will take the resistance of an armour-plate against shot as the square of its thickness—the rule which I still maintain is the right one with regard to armour-plates properly rolled—you would have for the 8 inches a resistance of 64, whereas you would have for the 6 inches a resistance of 36. Over that portion in which the useless dead weight of three single inches of iron is put on, there is, according to the law of resistance, an increase of only 3, which would make the resistance there only 39, so that we have all the weight of a 9-inch plate there without its value as a defence. Therefore we have an experimental structure built up here, in which masses of iron are put, useless to it as a defence, for the purpose of enabling the Government to get as large a number of experiments as they could out of the same shield. But if you take Mr. Hughes' design and make his target as a homogeneous structure, such as the Gibraltar shield was on that occasion, then you would put Mr. Hughes' shield of 8 inches with 7 inches of backing against the Royal Engineers' shield of 5 inches on 5 inches of iron plate—properly speaking $5\frac{1}{2}$ inches on $5\frac{1}{2}$—(altogether there were 12 inches of iron) representing a definite system. The Gibraltar shield has been abused, and called worthless, when it was not so. Properly constructed, it represented a definite system, that is, plate upon plate, a system which has been proved from the first and up to the present time to be valueless as a defence against artillery. I do not know whether this audience properly understands what Mr. Hughes' system of armour backing consists of. It is in the ordinary acceptance of the term a bridge rail, rolled in such a way, that when it is riveted up to the skin plate it forms a box-girder; and everybody knows that the box-girder is the strongest form in which iron can be put, and at the same time is the lightest. The real question is one which I should have thought ought to have been determined long enough ago, and one which official minds ought to be thoroughly acquainted with, whether that form of the hollow stringer does or does not represent the resistance of certain thicknesses of plate. Mr. Hughes took such means as a manufacturer had at his command to show that it did represent the resistance of a definite amount of armour. He took a strip of armour-plate of the same breadth as his stringer, and found by a weight of 1,500 lbs. let fall 15 feet upon this, that the permanent deflection was $\frac{5}{8}$ of an inch in the hollow stringer, and $\frac{5}{8}$ of an inch in the armour-plate. If that experiment is worth anything, it shows once and for all that a 7-inch hollow stringer is equal in resistance to a 6-inch armour plate. I should be the last man in the world, and I do not think Mr. Barnaby would charge me with the idea of putting a Millwall shield in the turret or on the broadside of a ship. But see what Mr. Hughes does with regard to his naval construction. He makes his inner series of vertical stringers, by riveting knees to every second or third stringer, constitute the frame of the ship. Then the skin plate is riveted on. That is his first line of defence. Then he rivets the outer set of horizontal stringers to the outside of the skin of the ship. That is his second line of defence. So that we get 14 inches of hollow stringer backing to support the amount of armour you want to put on it. Mr. Hughes does not say he wants steel or iron armour. If you take steel, so much the better for his system, because steel, if it does by its hardness resist the shot and keep it out, remains perfect. But if it does not succeed it is broken up and disrupted. There is a passage, and a very remarkable one, in Mr. Barnaby's Paper, which struck me with very great surprise. He is speaking about the shots fired against Cammell's compound steel-faced plate, and he says, "The first shot penetrated 8 inches and broke off the lower corner of the

plate, exposing the backing, and therefore breaking down the defence." Now with the Hughes' system, if you had exposed the backing, you would not have broken down the defence, beca e you would have had the two series of 7-inch stringers still remaining as a defence equal to an armour- plate of 12 inches of iron. I say, if you take into consideration the other fact mentioned, that we may possibly have, and certainly could have if we liked, a large shell carrying an explosive charge to burst within the backing, you would then have, if your armour-plate was blown off, a backing of great value, on Mr. Hughes' plan, instead of a backing of so much timber, altogether valueless, as in the Admiralty mode of construction. Now, again, with regard to the question of weight on board a ship, which is an important point. The Hughes' system allows the naval architect to dispense with the ordinary frame of the ship, because taking either the alternate, or the third, or any other intermediate hollow stringer of the inside series of the ship, by putting knees to these, in the very neat and admirable manner which Mr. Hughes has devised, you then make the backing itself, as I have said, the actual frame of the ship, and save all the weight of the ordinary framing. I say, therefore, that undoubtedly no other system of backing does give so much lightness. I say also it is unfair to make the statement that the Millwall shield is 960 lbs. to the square foot, when a target of those dimensions properly constructed for land forts and estimated in the usual way would not be more than 645 lbs. I should like to say one thing with regard to the plate upon plate system of the Gibraltar shield, which was the competitor with Mr. Hughes' shield in 1868. Taking the Millwall shield as having 9 inches of iron, the Gibraltar had 12 inches of iron, nevertheless in two shots the vibrations against that shield were so great that no less than 14 of the through bolts were broken off and sheared, and the gun practice was discontinued, as the third or fourth shot would have disintegrated the target entirely through the vibration. Then the Gibraltar shield was repaired and shot at again. You must remember in those days the 9-inch gun, carrying a shot of 250 lbs. weight, was the heaviest gun we had practically, or at least that was the gun the shields were intended to encounter. After the Gibraltar shield had been strengthened, had had new bolts, and had been set up again, the first Palliser 250 lbs. shot which struck it perforated the shield entirely, and blew strips of the armour-plating back into a curve, as is represented very well indeed in the official report on the trials. I think if any gentleman will look at the photograph of the effect of that shot he will comprehend clearly what is the value of the plate upon plate system. I will also point out another very serious item, to my mind, with regard to the use of the Hughes' system on board ship; and it is, that the hollow stringer girders being filled with teak, the cushion of wood just gives so much resistance and so much elasticity to the backing, that the armour-plate vibrations which would be caused by the contact are prevented. Again, in the wood backing which exists behind these armour-plates in the present Admiralty mode of construction, a shell bursting there would set fire to that teak backing, and the space between the skin of the ship and the armour would become a regular furnace: by the slightest indraft through the shot hole the wood is blown into flame, and the ignition of a large portion of that wood backing is inevitable. In no case where shots have been fired—and I have seen a number of shots fired at Shoeburyness—has anything like a breeze existed, but the backing of the targets has ignited, and has had to be put out. In Mr. Hughes' system the fire would be limited to the teak filling of the one particular line of hollow stringer affected; but I do not wish to take up any more of your time on this occasion. I think I have said quite enough to defend Mr. Hughes' system, in which I have long felt the greatest interest; and certainly I am at a loss to conceive how it is that nobody seems to know what the value of this system is, though it was so fully and so publicly demonstrated in the year 1868.

Mr. J. Scott Russell, F.R.S., V.P.: Mr. Chairman and gentlemen, I am sure I express the feelings of nearly all the practical men present at this meeting, when I say that we are extremely indebted to the Admiral for having given us in a short summary, the sum total of all the results derived

from his lifetime of practical experience on the subject; and we are extremely indebted to Mr. Barnaby for giving us the results of his official experience. The summary of their opinions, upon these two points, I think forms one of the most valuable contributions to our Transactions. In this discussion, and in those papers, the three points that appear to me to be raised, and are of the greatest importance, are the following. The subject of steel for armour is inevitably introduced, and must be introduced into any discussion of this kind at this moment. The relative question as between steel and iron plates is inevitably introduced; and the third point is one to which I have always called great attention, but which has not always, I think, been considered sufficiently important or yet discussed in these meetings, which is the element of structure of the ship as compared with armour. Now in the point raised last, namely the advantage of those hollow girders with wood as a backing to the armour, the one obvious point which I see, but which has not been I think much alluded to, is this, that supposing a new ship to be built, and wisely designed, all this portion, whatever may be its value in resisting shot—all these additional strengthening girders introduced in the diagrams before us, might be so utilised as to form integral parts of the structure of the ship; and therefore their strength would be added to the ship, as well as added to the armour. I think that is a point of great importance. Perhaps like Mr. Hughes, who thinks his shield was the best possible, I might be expected to state that my shield was the best possible, only I am afraid that we should all be of an unanimous opinion, and that every other fellow would say, my shield was also the best. Therefore we will not argue that point, but I will merely call attention to this, that I consider Mr. Hughes' shield better than mine, and the reason is, that whereas I did utilise a great number of longitudinal stringers in between the plates, for the purpose of incorporating the strength of the ship and the strength of the plates, and making them both help each other in their respective work; Mr. Hughes, I think, goes much further with that principle. If I were building a new iron war-ship I think I should be very much disposed to incorporate these girders of his in a twofold structure as part of the ship and as part of the backing, and that therefore, instead of adding them all to the cost and weight of the armour, we should divide it between the armour and the ship. Now there is another point which has not been alluded to; and here again I am obliged to go back to old experience—as far back as the *Warrior* class, for example. A great deal has been said to-day about the advantages and disadvantages of this mere backing of wood, but it does appear to me that what has been discussed to-day gives us a very fine opening. The whole six ships of the *Warrior* class were constructed with a moderately thin armour at that time, and a very thick backing. I do not know what other people thought, but I intended the *Warrior* class, at the initiation of which I had a good deal to do with them, to have a second coating of armour whenever the bore of the guns became so great that a second coating of armour was wanted; and therefore, it appears to me, in order to make the *Warrior* class efficient ships of the present day, you have only to remove their present plates, to dig out a certain quantity of the thickness of the wood, put in a whole series of these stringers, as they have now been described, along with wood backing, on a second layer of plates, and you will at once have a most efficient ship, of very ancient date, but almost as efficient as most ships of the present day. It appears to me also that it is very desirable to take the following considerations with us, and not to adopt any exclusive system, in the following manner. Steel is gradually coming forward. In what manner? Not in hardness, because it had always got that quality; but steel is gradually coming forward in acquiring the quality of toughness. Now I think in our minds we should distinguish very accurately between what mere hardness does and what gentle pliability or softness does. I like steel very well as an outer skin for hardness, but steel has an abominable quality that I do not know if you have studied; it has this abominable quality, that a thing called a cone in the inside of the steel rushes out from it whenever it feels concussion on the outside, and this cone flies away, and that is

all the resistance the steel can offer; whereas a tough material, yielding behind, will go backwards along with the shot in a large mass, without a cone, to a moderate distance, and in doing so will give enormously greater resisting power than this small conical bulge driven out. That I would call your attention to. Finally, I think it is of great consequence that the structure should gently yield—not in the way of breaking, or of cracking, or of splitting, but should gently yield in the way, if you like, of being dinged in, because I think if a ship came out of a naval engagement all covered with dinges and dimples, but not with cracks and holes, you would agree with me and say that that ship had been perfectly successful, whereas those that were split and open you would say had been defeated. I believe, therefore, that we must in the first place introduce the incorporation of the armour and the structure of the ship into each other. Secondly, I think we must get for our armour, whether you call it steel, or whether you call it iron, as plastic and yielding a substance as we possibly can, and the moment your steel arrives at the plastic perfection at which I hope it will arrive, then, if you please, we will call our armour-plates not iron, but steel. At present we do not know what steel means, and do not know what iron means; but by and by we shall know exactly when to use the one and when to use the other. I think that is all I have to say on the subject.

Mr. J. D'Aguilar Samuda, M.P., V.P.: My Lord and gentlemen, I did intend to say a few words on these papers, but I have had so short a time, in common with everybody else, to look into these elaborate statements, that it is quite impossible that justice can be done to them on the present occasion; and I hope in any observation which I make that will be taken into account. I must confess there are two things which strike me very much in the consideration of these two papers. First, in Sir Spencer Robinson's paper I notice the comparison is drawn between the corresponding amount of weight of solid armour in the one case, and of backed-up armour with these bridge rails in the other; and that a conclusion is drawn favourably to the latter construction. Without going into the detail, as it has been criticised by Mr. Barnaby, I would refer to this criticism to show the difficulty that one has in dealing with these matters upon the spur of the moment. Then in that able defence which was made for Mr. Hughes' plan by Mr. Mackie, as I understood him, although he advocated the great goodness which it contained, he threw it overboard altogether, and said he would not for one moment think of introducing it into the armour of a ship; and therefore we have these difficult matters to reconcile between the advocates of the system; one proposing it as being superior to everything which has gone before, the other throwing it overboard as being totally unsuitable to introduce into a ship.

Mr. Mackie: I think I ought to explain what Mr. Samuda seems to have noticed. Perhaps I did not make myself clear, speaking so rapidly as I did, and it was not likely that I should think of everything at the moment. But what I intended to have said was that I did not propose to put such a Millwall shield on board a ship as was shot at in 1868, not that I did not approve of Mr. Hughes' system of construction for ships. The Millwall shield of 1868 was strengthened by order of the Royal Engineers by putting several 12-inch girders behind it, which was very much like strengthening a soldier by putting an additional knapsack on his back. Such a mode of strengthening added greatly to the weight without adding greatly to the resistance to penetration by shot: it had this value in a land fort, that more weight tends to keep a shield in position in an embrasure. We do not need this weight in naval construction because the ship itself is part of the entire structure. I did not say that the hollow stringer backing was not suitable for ships. Mr. Hughes' original shield was composed of an 8-inch plate with a 7-inch backing, and such a structure, worked in as it would be, as an integral part of the hull, I consider quite suitable: and the strongest and lightest of any system yet brought forward.

Mr. J. D'Aguilar Samuda: I do not wish to go very minutely into these matters. Again, I did

not gather that that which was being advocated had been really submitted to the practical test of being fired at, to enable us to deal with the matter from a practical point of view. I must confess my own impression is very strongly against the supposition that a mixture of the bridge-rail backing can be equal to that of the solid armour. It may be a prejudiced view I possess, but it is within the recollection of many who are at the Admiralty, that so far back as quite the early days of armour-plating, I myself submitted to the Admiralty a plan of making the construction of a ship the armour itself, because I always held the view (which I believe has been pretty generally assented to in this room) that the general result of the firing, with equally good plates, was to show the resistance to be as the square of the depth of the plates, and consequently all this building up arrangement I looked upon as being to a great degree waste of material. But the most interesting matter we have to deal with is to be gathered from the change of material that is dealt with in Mr. Barnaby's paper. That is something very importantly different. We are passing through a complete crisis with regard to the manufacture of iron; we are absolutely abandoning iron as fast as it can be done practically and with certainty, and exchanging it for steel—not in one matter only, but in every matter. Now I have no experience myself with regard to the results which have been obtained in armour, that is, the armour immediately before us on the present occasion, but no one can read this paper of Mr. Barnaby's, which deals with actual experiments made, without seeing that the foundation is there laid for encouraging, still more than has hitherto been done, the substitution of steel for iron in the armour of our vessels. The only paragraph I would refer to is one which shows at once that there is a foundation laid to go upon which must ultimately result in getting more or less very considerable advantage from going in that direction. The paragraph is as follows: "At Shoebury a blow which has perforated a 13-inch iron plate has been delivered three times at a 10-inch compound plate 6 feet square, without any one of the three getting through the plate." Now the difference between a 10-inch plate and a 13-inch plate is practically that you are firing in the one case at a mass represented by 100, and in the other by 169—that is, according to my own notion of it—and yet the difference has been very considerably in favour of the thinner plate. That points out to you that whatever alterations we now require to make with regard to the enormous increase of the force of guns which we have to deal with, that that is and must be the direction in which our attention must be turned. I really think that whatever good may arise with regard to the advantage of the structure, as Mr. Scott Russell has pointed out, the structure of the bridge rail may be substituted to some extent, probably to a great extent, in the construction of the ship, in substitution for the usual description of frames which are put there; but yet you cannot disregard—and in that I think I differ with Mr. Scott Russell—the bridge rail as being no part of the armour, and as forming the construction of the ship; the moment you put it in such quantities beyond that which is necessary for the construction of the ship as really represents an additional weight carried to represent the armour of the ship——

Mr. Scott Russell: Half and half.

Mr. Samuda: Under those circumstances it does appear to me that every form, no matter how satisfactorily arranged, is an inferior form with reference to that which can be obtained by solidity; therefore I cannot think that the investigation of this matter will turn out as favourably as its advocates conceive. I do not like sitting down without making reference to the 5-inch plate which was referred to by Mr. Barnaby, which I understood to consist of a number of small hexagon plates put together by screws which entered into all the surrounding plates. Did not I understand that?

Mr. Barnaby: A solid plate with screws put in.

Mr. Samuda: I thought the screws were put in with the view of connecting the plates.

Mr. Barnaby: I should explain that the screws were put in with the view of limiting cracking, if there should be any. But, as a matter of fact, there was no cracking.

Mr. Samuda: Then I must have misunderstood it, and I did not want to refer to it in that view. My object in referring to it was this, that I always conceived, and still believe, that the greatest difficulty we have to contend with in armour generally is the probability of cracking taking place from the bolt-holes; and as far back, I think, as 1862, I myself, with the view of avoiding that, adopted something very similar to that plan. I patented it, but it is public property, as it is beyond the term now. By that, I proposed that no bolt-holes whatever should be made in the plates, but that the plates should be connected at their edges one to the other by screws which entered half into each plate, so that there was no place in the centre of the plate for the cracking to start from. That does appear to me, to some extent, what was intended in this plate. I fancied I gathered from Sir Joseph Whitworth's explanations to me personally that one object he had in view was to put the plates together—which were octagonal in form—with steel screws screwed into the junction of them, and thus increase the resisting power of the armour, and also to prevent the necessity of having bolt-holes in them. That was a mixture of plates put together in this way. I am sure that we must all feel greatly indebted to both of these gentlemen for the papers they have put before us. I think they are papers which will be extremely valuable to us as forming part of our Transactions. I feel that I have been able very imperfectly to deal with the subject, from not having had the opportunity of studying it before.

Captain Brown: With reference to the second experiment mentioned of Sir Joseph Whitworth's circular shield, on page 32, I wish to ask, was the projectile that was employed a regular service 9-inch chilled projectile?

Mr. Barnaby: It was, but it was planed down to fit Sir Joseph Whitworth's gun.

Captain Brown: Made hexagon?

Mr. Barnaby: Yes, and then loaded to make up the 250 lb. weight.

Captain Brown: If you look at that plate you will see how much of the projectile is cut away; Now there have been experiments made at the Royal Arsenal Laboratory, showing the enormous additional strength the outside coating gives to the interior. If you cut away the whole of the outside and then fire it, it certainly is not in the same condition it was in when originally cast. It is hardly fair to treat it in that extremely cruel way. There is a good deal even of the chilled part cut away. You call its original weight 250 lbs., but if you cut the outside part away, and then add loose weight to it, and fire it in that extraordinary and crippled condition, and then exhibit its remains in a glass case, I think it is very hard on the chilled projectile. I do not believe chilled projectiles are as good as steel ones recently made, but I wish them to get fair play. Now, let me say one thing to show that I do not wish to be unfair to the steel projectile. I believe that it has hardly had justice done to it in the case of that 9-inch projectile, which was fired three times. I understood Mr. Barnaby to say it was fired three times at a 9-*inch plate*, and the third time with a charge of gun-cotton. I believe, however, the plates which were fired at the first two times were 12-*inch plates*. Are we to understand that the third was a 9-*inch plate?*

Mr. Barnaby: So I am told.

Captain Brown: I believe I saw the steel projectile of Sir Joseph's after it had gone twice through a 12-*inch plate*, so that it was fired thethird time through a 9-inch plate carrying a gun-cotton charge. I am anxious to see justice done to that—but it is right to point out that I do not think that the cut-down shot in the Whitworth trial represents the chilled projectile at all.

Mr. Barnaby: It is my duty to defend the use of that projectile. The reason why the Woolwich projectile was taken for the purpose was, that it might have been said Sir Joseph Whitworth, having

made his own, had purposely taken bad cast-iron. His wish was that he might have a thoroughly well-made, chilled cast-iron projectile.

The President: You will have a reply, Mr. Barnaby.

Admiral Selwyn: I wish to ask for a few moments' attention from the audience to speak on one or two points in connection with these papers. I think Sir Spencer Robinson would admit that a great deal of his paper would be modified, if he were perfectly certain that the results here shown with steel could be continued and extended, and that we should not first have to discover the best method of arranging steel to resist a blow, and the quality and tempering of each part of the plate, if so made of steel, before we begin to decide either on the backing or on the form of support behind the plate in any way whatever. It is quite clear that as yet we have only advanced a very small way with steel. We have consented to try steel instead of iron, that is to say, to try a good iron instead of a bad iron (it does not amount to much more than that), and we have not yet defined what is the difference between steel in the molecular state—the crystalline state, something resembling cast-iron of a better material, behind a thin sheet of properly tempered steel in the face (and I believe the thinner that may be, the better it will be found to act), as in the anvil, keeping together this crystalline plate by a proper skin surrounding the whole plate, not alone on the face, approaching to the qualities of best tough wrought-iron. Those are the conditions under which the anvil resists blows, and the reason why is not difficult to see. The cone of dispersion which Mr. Scott Russell speaks of, I, on the contrary, venture to regard as one of the reasons why a blow struck on the face of any resisting structure is absorbed and distributed. I say that there are two ways of distributing and absorbing that force; the one, when, having a soft plate and a moderately resisting backing, you distribute waves of pressure in the soft plate, and you crush up the atoms of the wood backing, in which case you slowly absorb the pressure; the other, in which the blow received on an elastic and perfectly hardened surface distributes through the molecules of the steel itself a cone of dispersion so much larger in diameter at its base, as to distribute again to whatever may be behind, over a still larger surface, the blow received. Those are the two methods in which I can conceive force so applied can be distributed and absorbed. But it is found that when we desire to resist blows, that a hard external surface, provided we have a perfect material like steel, is the best mode, and that it leads, under certain conditions, to the breaking up of all projectiles. No increase of velocity, leading to a corresponding increase of force, would have much value unless with it we were able to get a material in the projectiles so far superior to what we now have, as that that force could be safely applied to such resisting surface without the breaking up of the projectile itself. Now the recent experiments, referred to by Mr. Barnaby, show that we have reached that limit with steel already, that in certain instances the projectile is unable to do anything in the way of penetration, and its whole blow is absorbed in breaking up itself. This is a most important advance, and will, no doubt, lead to the reconsideration of the whole of the structure. I think, as I long ago said in this room, if we followed the conditions under which the anvil resists, changing our material according to our modern lights, we shall arrive at the best resisting armour. That the structure behind it ought to form part of the general structure of the ship, I think is sufficiently obvious to have long ago received full acceptance. That the part of the general resistance which it is desirable to obtain from coals, as well in ironclads as in the vessel on the wall, the *Hecla*, can be obtained, is also, I think, sufficiently obvious. We have to make yet another advance, that is, to diminish the surface of the target, which we think ought to be as low as possible. That I propose to talk of afterwards, and I will only further say I think the profession to which I belong has very great reason to thank the gallant admiral, Sir Spencer Robinson, for the compendious way in which he has put together a great deal of information on what has passed, and also thank Mr. Barnaby for the knowledge we have authoritatively received of the results of modified experiments in steel. I hope

that they will be carried much farther, and pursued with the greatest energy, since on their success or failure will depend the whole of the most useful changes to be arrived at in the future construction of our war-ships.

Admiral Sir Spencer Robinson: Mr. Chairman and gentlemen,—I am extremely anxious to have two or three minutes of your attention to explain the circumstances that Mr. Barnaby stated with extreme kindness and consideration for me. I have taken the greatest possible pains (I need not say that) to be accurate and fair in everything I have stated. I should be ashamed to stand before any gentlemen, let alone before such an audience as is here, if I had not taken the greatest possible pains to assure myself of every fact that I have laid before you. I should be perfectly ashamed of myself if, for an instant, I were capable of such conduct, and I think every gentleman in this room with whom I have come in contact will be ready to say that in anything I have stated, either publicly or privately, there is no intentional unfairness whatever. There is a difference of opinion, and a very considerable difference of opinion, as to the weight of the Millwall target, and that bears upon my argument most materially. If the Millwall system of backing is, as I suppose it to be, stronger with the same weight of iron, I think I have established a case, if not for the use of the Millwall system of backing, at least for inquiry. But if I have been wrong, and if I have unfairly stated the weight of the Millwall target—understated it—then I think my whole argument falls to the ground, and that I am standing before you in a very unpleasant position. However, I think I shall be able to show you most satisfactorily that in what I have done about the Millwall target, and in the weights I have stated as the weights of the Millwall target, I am as nearly right as one can be upon such matters. There may be, I daresay, some little paring away here, or augmentation there, but it is impossible that one can be perfectly accurate after so many years have elapsed, when the drawings and weights of all that constituted the Millwall shield have disappeared, and one has to trust to people's memories. I only claim for myself an approximate degree of truth in what I have stated. The Millwall target was supported on the ground in the strongest manner that any target was ever supported on the ground, in the strongest manner that any target could be supported; and in taking the weights of the Millwall target, I took them entirely independent of the supports. I did so with the *Bellerophon*, and I did so with the *Hercules*, and I did so with every target whose weights I have compared before you to-day. I therefore took the Millwall target without its supports. But this is not the whole of the case; the fact is, that though my weights are strictly and literally correct, if I take so many inches of iron plating, so many inches of hollow stringers, and so much wood, my weights are as correct as they ever can be, or as they are for any other target, because, as you all know, every cubic foot of wood does not exactly weigh alike, and every cubic inch of iron does not exactly weigh alike. Some little margin must be allowed for that. The fact is, I have taken what the Committee before whom the Millwall target was fired and tried, stated. They said the main part of the target was 27 inches thick, and that another part of the target, near where the port hole was cut through which the shot passed, was 18 inches thick, and I concluded that I could not be wrong in taking for the weight of the mainport of the target a 9-inch armour-plate, backed by those double stringers, one vertical and the other horizontal—adding together the two plates which formed the skin, and adding to that the covering plate which goes behind the vertical hollow stringer which formed the frame of the ship. It is perfectly true I was aware at the time I made that calculation that this target had the very strongest supports, but I am not in the habit, nor do I believe is anybody, in reckoning the weight of a target, of considering its support. I have this to say: this target was so strongly supported, it was so heavily laden with iron in various places, that I think it would not be fair to say that my conviction is that the whole of this extra weight in support was thrown away, and afforded no sort of support to the resistance of the projectiles. I think

it would not be fair if I did not say that when I wrote this paper a few days ago, I had not in my possession a Report which has since been brought to my notice by Mr. Barnaby's kindness. I had gone to the War Office, and I had made every possible inquiry on this subject, and I had been assured that there was no paper or photograph similar to that which Mr. Barnaby has got in that book, and which I have also, but unfortunately have left at home, or I would read an extract or two from it. The truth is, I have found at the very last moment what would naturally occur to the mind of any impartial person, as well as to anybody who did not like the system of Hughes' backing, that this immense mass of weight, although not contributing to the essential features of the target, and though by no means what we could use on board ship, yet might from its massiveness, and the way it was disposed about the target, have contributed indirect support. That is the answer which I have to make—not to the accusation, because Mr. Barnaby was so kind and considerate as not to make any accusation against me, but to the idea that I see has rather prevailed in this hall, that the weights had not been accurately and perfectly stated. I thought Mr. Mackie explained the circumstances particularly well. I can only say that what I did for the *Bellerophon* and the *Hercules*, I did also for the Millwall shield. I took the length and the breadth of the stringers, I took all the weights, and compared them with the weights usually allowed for those things, and I venture to maintain that for that part of the target which would form the ship, my weights, 558 lbs. per superficial foot for a 6-inch plate, and 678 lbs. per superficial foot for a 9-inch plate, are perfectly and absolutely correct. At the same time I wish you all to understand that the other weights, although they would not be found in a ship, in my opinion contributed something to the resistance the target offered to the projectiles. There is one other point which I would refer to. I do not presume for one moment to discuss Mr. Barnaby's paper; I need not tell you that the length of time we worked together at the Admiralty made me thoroughly believe that in those subjects Mr. Barnaby has probably no equal—no one is so well informed as himself, and no one is more likely to put perfectly reliable results before you on these matters. He made an observation, not in the reading of his paper, which I do not discuss and which I accept, but as to which I am bound to say one word. He read from the volume of the professional papers of the Royal Engineers a paragraph condemning very strongly Mr. Hughes' backing. The case was, that an 8-inch plate against a 2-inch plate was tried with some 4½-inch plate and some of Mr. Hughes' backing, and some Cammell backing. That condemnation does not affect our argument, or what we have brought before you to-day, the least bit in the world. I venture to say it is entirely beside the question, because Mr. Hughes' backing was never tried at all. If you tie up one of my legs, and then tell me to dance a hornpipe, I should cut a very poor figure, and Mr. Hughes' leg was tied up entirely; that is to say, his principle being a principle of two hollow stringers used conjointly, one vertical and the other horizontal, only one was used against a backing of concrete, and the vertical stringer, in conjunction with the horizontal stringer, was not used at all. Colonel Inglis is a very great authority, and I am not going to dispute his views, especially as regards fortification, because probably he knows more about that than any man in England, but the whole argument he uses is that an 8-inch plate with 2-inch backing, put one in contact with the other, and tried against concrete, is the finest specimen of armouring that can be conceived. I might appeal with confidence to the judgment which Mr. Barnaby would form if anyone proposed to him to put an 8-inch plate before a 2-inch plate as the skin of the ship, and then to fill up with so many feet of concrete.

A Voice: Eight feet of concrete.

Admiral Sir Spencer Robinson: I want to know whether Mr. Barnaby would accept that as a proper system of armour-plating. I should like an artillerist to say a word or two about that. It may be right to use concrete, and the plate-upon-plate system, but the results of the firing which I have read are anything but encouraging to that mode of proceeding. It may be right to do that on

shore, but I maintain, and I say that Mr. Barnaby would agree with me, that it is not a system adapted to ships at all. Then various remarks were made by various speakers on the subject of the longitudinal stringer, and I think Mr. Samuda and Mr. Scott Russell said something about incorporating the armour-plating with the structure of the ship. That is practically what Mr. Hughes has done; and it is so obvious, as Admiral Selwyn has said, that I cannot conceive there should be anything but approval of such a system. I think I have disposed of the condemnatory report of Colonel Inglis as to Mr. Hughes' backing, because I have shown distinctly that Mr. Hughes' backing was never tried; and, therefore, that condemnation must fall to the ground. You would not for one moment listen to me if I were presumptuous or rash enough to talk to you about the qualities of steel, because there are gentlemen in this room who know as much as can be known upon the subject, and whose observation is daily directed to the progress of steel manufacture, and to all the results that will follow the production of steel. I hope there is nothing in my paper which led anybody to doubt for a moment that I appreciate as much as any man can the progress we are making in the manufacture of iron and steel, and the very strong hopes I have that we should succeed in getting all that we want from it; but if I made any reservation at all in that matter it was this, that up to this moment we are not in a position to say positively that the thing will succeed. We may hope for it, we may look for it, we may see it looming in the near future, but I am not positively bound to say that we will not take a system of backing iron plates, or will not consider it, or will not give it a trial, because in the distant future there looms before us a manufacture of steel which may render all backing unnecessary. All I ask your consideration for is this: if anything has been said to you to-day to lead you to believe that the system of hollow stringers is in itself a support to armour-plating better than the system of wood, and that sytsem of wood strengthened by a partial, not a complete, adoption of the very system of rigid backing which I am here to-day to advocate—cellular backing, at any rate—if anything has been said to induce you to think that though that system is not perfect, yet still something may be done for it; I only ask for your consideration. I want the question to be considered. I do not come here to dogmatise, or to say that this is right, and that everything else is wrong; but what I do say is, that I have produced before you reasons for the thorough consideration of this subject, and reasons why it should not be thrown aside into the waste-paper basket, as so many things of this kind have been disposed of after a lapse of time. I only ask your consideration of the subject. I was going to say a word about Mr. Barnaby's paper, but I will not. I have listened to it with very great satisfaction, and very great delight. There is only one point more, about the turret of the *Devastation*. I find that Mr. Barnaby is of opinion that I overstated the weight of the *Devastation* turret, because I put more teak into it. I ought to have stated the weight of the *Devastation* turret as containing fifteen inches of teak, whereas I said it contained seventeen inches. I refer to our Transactions, and I find a very able and interesting paper, which Mr. Barnaby put in himself, in which the thickness of the teak was given as seventeen inches. That may be a printer's error. I had another reason also for it. In a book that was kindly furnished me by the Admiralty it was stated to be sixteen to eighteen inches; I took that, but it is quite true that I have another paper in which it is only put down as fifteen inches. I thought in the course of building the ship those two or three inches might have been added. I am only explaining why I seem to have gone counter to what Mr. Barnaby thinks the absolute facts of the case. I have to thank every gentleman very much indeed for the extreme indulgence and great kindness which have been shown to me, in allowing me a much longer time than I had a right to in reading my paper, and for their extreme patience and consideration in listening to my paper.

Admiral Ryder: Before Mr. Barnaby replies, may I ask a question with regard to one of the diagrams? I want to ask whether the blow on that steel plate is represented accurately in position in the drawing—is it exactly or almost exactly opposite the bolt and the nut?

Mr. Barnaby: In answer to Admiral Ryder's question, I may say the projectile struck on the point which is indicated on the drawing there, upon the bolt, or just upon what you may call the bolt. With regard to the thickness of the turret wall of the *Devastation* to which I referred, and to which Sir Spencer Robinson has been good enough to refer in kind terms towards myself, I might have stated that the thickness of the backing was eighteen inches, because I suppose it might have been intended to make it then eighteen inches, but as a matter of fact it was made fifteen inches, and the skin behind it was not one and a half inches but was one and a quarter. In Sir Spencer Robinson's paper you will find he gives the thickness of the shield fired at at Shoeburyness to represent the turret wall of the *Devastation*, and he stated that that shield would represent a weight of 726 lbs. That is a small point, and I will let that pass, but there is another matter to which Sir Spencer Robinson referred, which is that the War Office authorities, in making their experiments at Shoeburyness, acted unfairly towards Mr. Hughes' system. The drawing which is given here shows that what they were anxious to do was to place upon a very strong wall indeed, first of all, a plate of two inches of iron, and then upon that to try three different plans of armour, all having the same weight per square foot. Upon the 2-inch plate at one end they put an 8-inch plate of solid iron with no backing. Next they put the channel irons and a reduced thickness of armour, making it per square foot exactly the same as before.

Sir Spencer Robinson: My lord, I rise to order. With your permission I want to say something. Mr. Barnaby began, if you remember, by criticising my paper; I have replied to the criticisms which Mr. Barnaby made on my paper, and he is now going over my reply, and criticising my reply. Now I only ask for fair play; one does not like to leave an adversary of such power and weight as Mr. Barnaby unanswered; and if he is to be allowed to criticise my reply, I trust that I shall be allowed to criticise his, and then where we shall end I do not know.

The President: I confess that it occurred to me when we decided to take these two papers as one, and to discuss them together, that possibly this difficulty might arise. I think with regard to the suggestion which Sir Spencer Robinson has just offered, it might be extremely difficult to say when this discussion would end. If they are to go on criticising each other one after the other it will take up a very great length of time. I think I shall best meet what I believe will be the feelings of the meeting if I suggest to Mr. Barnaby that in this somewhat unusual mode of proceeding the replies should not tread upon each other's toes.

Mr. Barnaby: It is somewhat difficult, my lord, but I will come to an end now abruptly. I should like to ask Mr. Hughes one question, and that is this: in the Russian ships of war Mr. Hughes' backing is employed behind the armour. Will he tell us whether in all those ships he employs *two* sets of his hollow girders? I understood from a remark that Mr. Mackie made just now that he always employed two. My impression was that that was not quite so.

Mr. Hughes: With regard to the Russian ships of war——

The President: I think, sir, the discussion is closed now, and it would be dangerous to have any more remarks upon it. However, before we part with these two papers, I cannot help saying something which I am sure everyone here will concur in, and that is, that whatever may be our opinions as to the replies or criticisms one upon the other, we are greatly indebted to two of the most competent men this country contains, for two very able papers on subjects of the deepest interest to the whole country. I am sure I may tender your cordial thanks, both to Sir Spencer Robinson and Mr. Barnaby, for the able contributions they have made.

ON NAVAL GUNS.

By C. W. Merrifield, Esq., F.R.S., Associate Member of Council.

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

I think it would be matter of regret if this meeting were allowed to pass over without any mention being made of naval guns. The guns, almost more than anything else at the present moment, determine the design of the ships of war: not only the gun which is to be carried, but also the gun which is to be met. For this reason I have ventured to bring the subject before you, although I would much rather that the task had been undertaken by some practical artilleryman.

The naval designer is concerned with the gun, which he has to provide for carrying, under two different aspects: first, as regards the safety and efficiency of the weapon itself; secondly, as regards the weight and the space to be provided, as a consequence of its adoption. In both aspects the English heavy ordnance is, I think, about the worst in the world, as well as highly expensive, having regard both to prime cost and to durability.

The guns, and consequently the ships, have been sacrificed to two conditions—muzzle-loading, and a defective system of rifling. It is true, that, at the time the gun rifled on the French method was adopted in our service, there was no system of breech-loading which could be considered as reasonably safe for large guns, but that has long since ceased to be the case; and even at that time I believe it would have been better to have expended a little inventive and experimental skill in endeavouring to produce a good breech-loader, rather than in wasting labour in the attempt to cover and conceal the defects in the muzzle-loader actually introduced into the service.

It is hardly necessary that I should go into a detailed statement of the advantages of a breech-loading gun, as far as naval design is concerned. It may be well, however, to make brief allusion to some of them. First, as regards the amount of room to be provided in a turret or casemate. For a breech-loader, all that need absolutely be provided, is a length of five or six feet in addition to the length of the gun, measured from the breech to a few inches in advance of the trunnions. There is no absolute need to cover the chase. In a muzzle-loader, on the contrary, in all ships which have hitherto been commissioned, provision has been made for housing the whole length of the gun, chase and all, within the turret or casemate, and some clearance is also in this case required for working rammers and sponges. It is claimed that a muzzle-loading gun is less complicated than a breech-loader.

This is certainly true for plain cast-iron guns, such as the old smooth-bore 32-pounders; but the difference in a great measure disappears when we compare built-up guns, such as are now in use. When the gun is made up of several pieces, the question is simply whether one or more of the separate pieces shall be removable or irremovable. On the other hand, it is quite certain that this simplification of the gun is purchased at an enormous cost; for the inconvenience and complication of all the fittings and surroundings of a muzzle-loader are enormously in excess over those of a breech-loader. In fact, a description of the gear and the processes used on board the *Thunderer* would read like an elaborate joke, if the subject had not been far too disastrous for jesting. A breech-loading gun, in other respects like that of the *Thunderer*, could be worked in a casemate, measuring from the rear wall or bulkhead to the rear side of the shield, fifteen feet, or four feet less than the length of the gun itself. It is not suggested that it would be desirable so to cramp the space, but it seems well to point out how much the space actually needed for working the gun is reduced by its conversion into a breech-loader. Then, again, the complicated arrangements for muzzle-loading not only require a much larger turret, or casemate, but also make it necessary to have larger portholes and other apertures leading into or out of the protected capacity, which is thereby weakened accordingly.

It should be observed that the present guns in the English service are confessedly too short to burn a proper charge of powder. So long as they remain muzzle-loaders, it is impossible to give them a suitable length.

As regards the weight to be carried, the breech-loader is but very little heavier than the muzzle-loader, so far as the guns themselves are concerned; but the weight of a turret necessary to hold a pair of muzzle-loaders, is, on account of the considerations just mentioned, half as much again as for a corresponding pair of breech-loaders. When the top of the turret is plated, this preponderance is much increased. One consequence of this is that the same ship can carry a much larger and heavier breech-loading gun than she can a muzzle-loader. The gain is not only in the matter of weight, but even more in the matter of space. In this respect, we have not only to consider the space actually occupied by the larger turret, and by its base on the lower deck, but also the loading chamber outside the turret, containing that wonderful sponging and ramming machinery, which, as it is said, enables a gun to be double-loaded without any indication that it is loaded at all. As regards both space and weight, matters of the most anxious solicitude to the naval architect, there is a great saving involved.

It is true that, by means of a singularly ingenious mechanical combination adapted to Her Majesty's ship *Inflexible*, the actual size and weight of the turret itself have been kept down, in a muzzle-loading arrangement, to the same quantities as would be required for a breech-loader. But this has been attained at the cost of a dangerous complication, and at a great sacrifice of space and convenience on the main deck. I think that no person endowed with an ordinary intuition of mechanical fitness, can fail to be struck with a sense of absurdity in the idea of poking the chase of an 80-ton gun out of one hole in the turret into another hole in the deck in order to be loaded. In finding fault it is good to be definite in particulars as well as in generalities. As regards particulars, the sacrifice of space and convenience on the main deck flat, and the sacrifice of flatness of the upper deck, are serious faults in the

design. Taking it in its generality, it is a marvellous, not to say perverse, stroke of inventive genius, to escape the more rational and natural expedient of breech-loading.

There are many collateral advantages which I only briefly mention here, because they concern the artilleryman rather than the shipbuilder. Without stopping to inquire whether the Joint Committee was right or wrong in its explanation of the accident to the *Thunderer*, it is quite certain that double-loading could not happen accidentally in a breech-loading gun. The drenching with water now considered necessary would also be needless, and its omission would not lead to the risk, which would now be involved, of the gun going off at a considerable depression, and sending a shot through the side of our own ship, from within outwards.

I now turn to the question of the efficiency of the gun; and here I observe that everything is sacrificed, partly to curing, and partly to concealing, the defects of the present system of rifling. There is a specious plausibility about the only excuse ever made for it, that it was undesirable to add to the pressure upon the gun, due to the resistance of the shot to forward motion, the further resistance due to its taking up rotatory motion more quickly than necessary. The attempt to secure this theoretical advantage, at best insignificant in amount, has caused the sacrifice of nearly every other good quality, and has introduced some sources of danger quite peculiar to itself. I need hardly repeat here, what has often been said elsewhere, that, as a shot cannot possibly fit a gun rifled with a varying pitch, there has to be a misfit, both on the loading and driving sides. The risk of accident from the shot and shell breaking up in the gun, from the studs getting adrift, or over-riding the rifling, are matters which primarily concern the gunner. What do concern the shipbuilder, are the weakness and want of durability of the gun, and the low velocity which it gives to the shot. The total friction, as Professor Reynolds has shown, is about double that which would occur with a uniform twist of the same amount at the muzzle. The strains due to its taking the rifling also come into play with increased effect when the shot has already acquired forward velocity and is in a weaker part of the gun. Two very momentous results follow—first, that it is neither safe to attempt, nor possible to get, a high muzzle velocity, and secondly that, for the same reasons, a sufficiently rapid rotation to allow of elongated projectiles being used cannot be secured. The result is a sacrifice of fully half its power as an armour-piercing gun. Armstrong has obtained a muzzle velocity of 2,000 feet a second, and more; 1,600 feet is quite common; while the Woolwich gun only gives 1,300 or 1,400 feet. The energy of impact varying as the square of the velocity, the relative efficiency is got by squaring these numbers.

Whatever excuse may have been originally furnished for the French system of rifling by the quick-burning, and therefore violent powder, formerly employed in our service, that has ceased with the recognition of the proper object of burning the powder charge, so as to get a pressure as nearly as possible uniform on the base of the shot throughout its journey within the gun. Supposing that no force is converted into heat, but that the whole work of the powder is expended in giving motion to the shot, the law connecting pressure and time is immaterial, and the velocity simply corresponds with the energy. But, for a given muzzle velocity, the maximum pressure in the gun can never be less than when the pressure is constant throughout the whole time the shot is within the bore, and the more nearly that

condition is fulfilled, the less the risk to the gun. When any part of the work is expended in heat, the equation between work done and energy imparted depends upon the reconversion of heat into useful work—a reconversion which is never complete. Moreover, the proportion of work mischievously converted into heat—to the detriment of the gun as well as to the loss of work—increases with the pressure, and the lost work due to this cause is likely to be much greater under varying pressures, than under constant pressures. This will hold true, although in different proportions, of the work spent upon heating the gases, as well as of that spent upon heating the gun.

It is worth while to give a numerical statement of the pressure needed in a particular case.

Neglecting friction, the pressure required to give a shot a foot long a muzzle velocity of 2,000 feet a second in a barrel 20 feet in length is about 4·63 tons * on the square inch. With a bore of 12 inches calibre, and a twist of one turn in 20 feet, the pressure required to produce rotation is about 150 lbs. Assuming the shot to be 2 feet long, this brings to a little over 9⅓ tons on the inch the pressure usefully exerted in propelling the shot. To this has to be added the pressure necessary to overcome the friction, both of the barrel and the rifling. I do not attempt any numerical statement of this, because it depends upon so many elements, that it would only be confusing to introduce it here. It would not, however, under good conditions, bring up the pressure to much more than 11 tons on the inch, in the case stated.

It will distinctly appear from this numerical statement, what a very small portion of the pressure is directly expended in producing rotation, and consequently what very small gain there is to be made, by endeavouring to secure that this rotation shall be imparted less rapidly than the forward velocity; and it must be remembered that, even when, as in guns rifled with a uniform twist, the rotation is directly proportional to the forward velocity, it is still by no means suddenly given. The really important increase of pressure involved in the rotation of the shot is the indirect expenditure in overcoming the friction of the rifling. This loss is less when the pitch is uniform, than in any other case.

I think three points have now been established: first, that the existing service gun is a bad weapon, both generally, and particularly as regards naval requirements; secondly, that it is of importance to replace it by a breech-loading gun; and thirdly, that the gun should be rifled with a uniform twist.

There has also appeared, incidentally, the importance of having a slow-burning powder, which will give nearly uniform pressure while the shot is within the barrel.

Not being a mechanical engineer, I feel some hesitation in discussing the actual mode of putting together the guns. There are, however, some points, in which the existing guns seem to be of defective or mistaken construction, and I think my remarks would be incomplete if I did not mention them.

* Let P be the pressure in pounds per square inch, l the length of the shot in feet, s the length of the barrel in feet, and v the muzzle velocity in feet per second, then

$$P = \frac{l\,v^2}{s} \times 0{\cdot}052\ ;$$

12 cubic inches of iron being taken as weighing 3·34 lbs.

In the first place, I cannot help considering a built-up gun as at best a makeshift, adopted on account of the difficulty or impossibility of getting a good one of homogeneous material. The difficulty has now, as I believe, been overcome; at any rate the time is certainly at hand for the production of reliable castings of steel, possibly compressed by Whitworth's methods, or improved by special arrangements for casting, or by subsequent annealing—a process which, as applied to large castings, does not seem to me to have received sufficient attention. It is objected to steel as a material for guns, that it is brittle, and, when it gives at all, that it bursts suddenly, and without warning. I am unable to see that this is any real objection, provided the stress, under which it is safe, is in excess of that which the gun need be put to, or of what existing guns of the same weight will bear.

Next, I have never been able to look upon the system of shrinking on the coils, so as to gripe one another, as in any way satisfactory. I cannot understand any one way in which it is better than an exact fit, while there are many respects in which it is objectionable. The heating of the gun is always greatest on the inside, and this secures, at any rate after the first shot, that the outer coils, if only close-fitting, shall at once gripe the inner coils, as soon as these begin to expand under internal pressure. I have a suspicion, too, that the grip is not permanent; but that, especially with the heating and concussion of firing, it soon relieves itself by molecular re-arrangement, or stretching. I am quite aware of the theory, that materials do not take a "set" under stresses which are within the limits of elasticity; but I am also aware, that the assumption of the existence of such limits is one of purely theoretical convenience, and I believe that, as applied to soft wrought iron, it is a mere fiction. For glass, or hard steel, the theory is certainly approximative, and represents nearly, if not exactly, the actual state of things: not so for soft iron.

I think, also, that soft iron is far too extensible a material for the outer coils. So far as concerns strength alone, the outer coil, and not the inner tube, is the place for steel.

I had it in view, in writing this paper, rather to discuss the defects of the guns now in our naval service, than to go into the question of the *Thunderer* accident. But I think it right to say, that I am unable to accept the conclusion, that the accident was no fault of the gun. I believe it was entirely the fault of the gun, and that the only security against the recurrence of such accidents is to discard the present Woolwich gun, in favour of a better system.

DISCUSSION.

Admiral Sir R. Spencer Robinson, K.C.B., F.R.S., Vice-President: My Lord, I think it a great pity that this question should not be discussed. Although I am not at all prepared to enter into the scientific question that is put before us, yet I can give one or two opinions which I hope will set the ball a rolling, and induce gentlemen more thoroughly competent to deal with this subject to give us their opinions on the matter. As far as what I may call the non-scientific part of it—that merely practical part which relates to naval officers, I am entirely at one with Mr. Merrifield in this sense, that I wish extremely, for the purpose of naval attack and defence, to have the gun a breechloader. What I thought Mr. Merrifield was so right in was, that we should gain so enormously in the

construction of our ships, and the power which we could develop by breech-loading, and that the difficulties and obstacles in the way of making a perfect breechloader have been so far removed, though they were very weighty at one time, and perfectly sufficient to deter us at the moment from embarking in the manufacture of breechloading guns. I do think those obstacles are now removed. I think the discussion we have had to-day on matters relating to the manufacture of steel, and developing various new forces from iron, has helped us considerably, and I think we have come to the time when we might say with perfect safety and confidence that our gun manufacturers in England can deliver to us perfectly safe and useful breechloaders. I hope that idea will prevail in quarters of more importance than my own mind. There have been other points referred to by Mr. Merrifield which I confess are, perhaps, rather speculative, but it is extremely difficult for anyone who has read the account of the witnesses who saw the catastrophe on board the *Thunderer* to reconcile the report of the committee with the evidence given. There is in the weight of the evidence something so remarkable to a practical man that I think I may be excused for referring to it. The captain of the ship and others watched the progress of the firing. The captain of the ship, and others most competent to give a positive and decided opinion on the matter, declare that they saw three projectiles leave the ship.

The President: Three?

Sir R. S. Robinson: Four projectiles should have left the ship. They declare they saw three leave the ship, and one—it was perfectly well known to everybody—did not; that was the projectile from the gun in the after turret, which did not fire at all: that was acknowledged and admitted. But the great question, and it is a question of evidence, is this: Is it a fact that the gun was loaded twice, or is it a fact that the gun was discharged, and that the shot was seen by competent witnesses to go through the air and strike the water. There is contradictory evidence. There seems to be nothing in this world that is not contradicted in one way or the other, and it seems to me if you really want to get at the truth you have to go prodigiously deep in the well, and even then you may not find it, after all. But as far as the evidence goes, it seems to me the conclusion we should come to is, that that gun was discharged. Now if that gun was really discharged, and it was not the two cartridges and the two shot that burst that gun explosively, there is a very serious matter of weakness in the gun, and that serious matter refers itself not only to the construction of the gun but to the rifling of the shot, and the risk of a jam in the bore. But I have occupied your attention a great deal this day, and it would be very unfair of me to continue these arguments that have occurred to me on the matter; besides which, I see before me gentlemen perfectly competent to discuss all the points of this very important subject. But we must admit that Mr. Merrifield, in his most able and interesting paper, has put before us matters that we cannot ignore, and which we must not ignore. For the advantage of our shipbuilding, we must not ignore the fact that a breechloader is, in many respects, far better adapted than a muzzle-loader to naval warfare, and we have no right whatever to ignore this fact; that the explanation of the destruction of the *Thunderer's* gun is extremely open to question, and the decision arrived at may not be an accurate one after all; and lastly, this is not the first time—this is not the hundredth time—I have heard, and been told by men whose judgment and value, by men whose mathematical acquirements I rest on as implicitly as it is possible to do, that the whole system of our rifling is an erroneous one, and that accidents of this kind might and would happen inevitably. The only comfort I have ever had from that unpleasant reflection has always been the assertion of those who so ably and well have supplied us with our artillery, that it was perfectly impossible, even if they were wrong in the system of rifling, and considering also the liability I have adverted to, of a jam taking place, that the gun should burst explosively. I am quite sure if anybody has ever listened to those who advocate the present system of guns he has been told, over and over again, "There

may be something in what you say, but of this you may be quite sure, that no gun of our construction on the Woolwich system will ever burst explosively. Unfortunately, gentlemen, we know that that is not so now. I think, therefore, what Mr. Merrifield has brought before us to-day is doubly interesting, and ought to be the subject of our most serious consideration.

Mr. John MacFarlane Gray: My Lord, I quite agree with all Mr. Merrifield has told us about guns, in his usual clear way. He has put his paper about guns, in such a way that it will be very useful to many of our members, and also to others. With regard to the *Thunderer* explosion, I at first felt very much as Sir Spencer Robinson did just now, that the gun had been discharged. I was very positive about that; but I was converted by reading a letter: that letter was so well written that it shook my faith, probably partly because I like to look at things from the other side to that in which the party wishes to put them. It is a fortunate thing for Britain that it did burst, because it may lead to an improvement in our guns, and, I think, if it does, that improvement will be purchased cheaply, even at so great a price. With regard to new guns, there is at present preparing to be presented to our Admiralty a gun made upon a new principle. The inventor is willing to take such guns as those we are speaking about, to bore out the rifling and make them twice the strength they were before, adding nothing to the weight, not putting them in the fire. The plan is very simple, and, whether it will be perfectly successful or not, I think it is very pretty. They take out the rifling; they then put in a machine, the details of which I am not at liberty to tell you, but it is sufficient to tell you this, that it is somewhat like a Dudgeon tube expander. They put that in a common lathe, and after working it inside the bore of the gun for a day or two, the bore of the gun is enlarged—perhaps a 10-inch gun is enlarged to a $10\frac{1}{4}$ inch or a $10\frac{3}{8}$ inch—but the outside of the gun is not enlarged. They have carefully callipered the outside, and not enlarged that. What they do then is this: they put a condition of permanent stress upon the gun from the inside outwards; the pressure which they finally apply to the inside is somewhat greater than the pressure which the powder will exert, and consequently, when the gun is under the pressure of the powder, and is discharged, there is approximately no stretch in the gun at all. If this plan is successful, I think it will to a great extent reduce the heating in firing.

Captain Scott, R.N.: My Lord, I wish to say a few words on this subject. I do not think the mode of expansion which has just been spoken of would tend to strengthen a gun, but, on the contrary, it would destroy the elasticity of its metal. Now to pass first to the turret. A great many mistakes have been made with respect to it. Mr. Merrifield has said that he does not wish to curtail the space in the turret. Having had some experience in the matter of mounting guns, I can safely say there is not inside the *Thunderer's* turrets too much space to work the guns; but there is space enough for the officer directing the firing to have all the operations of loading, sponging, &c., under his eye; and thus to work a gun safely and efficiently. It has been constantly asserted in the papers that there is not space in the *Thunderer's* fore-turret or after-turret to work 38-ton guns entirely within them. There is, however, no difficulty in mounting 38-ton guns so as to work them *within* the turret, and by working them within the turret every operation is under the control of the officer of the turret. Can you expect that guns can be efficiently worked when the greater part of the gear is out of sight, and when the officer in the turret cannot see what the loaders (on the main deck) are doing. How can there be efficient control under such a system? I do not think the great danger of loading the gun in such a way has been fully apprehended. If the gun were to explode in such a position while loading, as the Duke of Somerset pointed out (and it is always the unexpected that happens), nothing could save the ship. The whole force of the explosion would go downwards and destroy her powder magazines, and certainly sink her. Again, you want sufficient space in the turret for long slides

for your guns to recoil upon. These slides form the strengthening ties of the turret. If you take them away, you take away a great proportion of the strength of the structure. Up to the time of the 35-ton guns, the whole of the heavy guns were mounted upon the principle worked out by me with the assistance of draughtsmen and others. One uniform system had been carried out in muzzle-loading, and there had been no accident with any gun. The reason is that when the charge was put in and the gun fired, it remained at recoil, and if it did not recoil it was clear that the charge was still in the gun. There is one very great advantage the breach-loading system possesses over muzzle-loading, and that is, that on board ship the gun is fired invariably with some elevation, because the shot commences to drop the moment it leaves the gun, and the gun consequently comes in the very best position for loading. No change is necessary, so that in firing by night the crew can safely go on firing, knowing that the gun has the right elevation, but directly the elevation is altered for loading the gun, an element of error is introduced. The fewer operations you can have, and the more you can simplify them, the greater is the security. I have mentioned before, and I still believe the Prussian officers, who are not willing to say much about the guns I saw on board their different vessels, are not satisfied with their heavy breechloaders, in fact, however well a breechloader may answer in firing small charges of powder—and so far I am entirely with Mr. Merrifield, and think that all light deck guns should be breechloaders—when you go beyond 70 pounds, and fire enormous modern charges which greatly heat and expand the gun, I think it very likely you would find something go wrong, and the slightest escape with these immense charges of powder would prevent you from closing the breech, and certainly would disable the gun. There is another point which I would refer to. To load the gun at the breech you would either, as before indicated, have to alter the elevation, or load it uphill, but in loading at the muzzle there would be no difficulty in first entering the charge, and then ramming it home by a stiffened chain, as some of you have seen done in the Arsenal. That chain is capable of great improvement, and of working with much exactitude; or having first entered the shot, you may place ropes on the ordinary rammer and ram home the charge easily. Even with the 81-ton gun there would be no difficulty. I do not wish to detract from the present plan. We know the high renown of the firm which has mounted such guns in the *Inflexible*.* We know the great things they have done for the navy of England; but at the same time I would point out that the simple mode which has been carried out throughout our navy, could be extended over to 80-ton guns wherever the space was not too contracted. The time of working the *Devastation* 35-ton guns by hand is shorter than that in which the *Thunderer's* guns can be worked by steam. I believe if the after-turret were fully tried, it would be found that the same result would hold good there—that the quickness of the after-turret would exceed that of the fore-turret. It is incorrect to say that because a gun is three tons heavier, therefore, as it was replied to me, steam was required in the one case and not in the other. My reply was, let me mount the 38-ton gun, and add steam, or let me else put the steam to the 35-ton gun, and I will show you that, on the one hand, while you will be able with a trained crew to work with steam (and I hold that only a trained crew can work with steam), on the other hand, with a hasty levy of men should the steam fail, you can at once fall back on the more reliable—the hand appliances. Without hand-power when at anchor or under slack steam, you would have to ask your enemy to be good enough to wait until you got your steam up. Certainly you would be taken at a very great disadvantage if you were in such a condition. Most of us who have had much to do with machinery—and I suppose I may call myself somewhat of an engineer—know that a valve fails

* This ship's turrets are covered with very thick armour, and are proverbially contracted in diameter to save weight; hence her 80 ton guns cannot be loaded except by such special arrangements outside the turrets as have been so ably devised at Elswick.

in a moment, and you cannot tell what has gone wrong. In the different trials that took place on board the *Thunderer* much time elapsed before they could make the guns work efficiently. What occurred with the 100-ton steam crane, some of you may not know. The War Department wanted to get the 81-ton gun to Shoeburyness. It was on the wharf ready, but when the men wished to use the crane it would not work, and they could not get it to work for a fortnight. Nobody from that time to this knows what was the matter with that hydraulic crane, which is actuated by steam. The turrets of the *Thunderer* are just the size required for the crew to work the 38-ton guns efficiently, when using hand power, but if you reduce the size of turrets, you will have to come to breechloaders, and whether you will get those advantages that Mr. Merrifield supposes, or not, I leave him to decide. I will now turn to the burst gun and the Woolwich rifling. When the accident was reported it seemed to me that it might very well have happened by the shot jamming when it moved forward. The first part of the rifling in the 38-ton gun is practically straight, and then comes a curve. The shot might slip forward until it came to the curve, across the gun, as it were, and having a good deal of velocity, it might not follow the curve, as has been the case on several occasions. I have myself seen marks upon the guns which have indicated that the projectile has not taken up the rifling, but it has slipped over the edges of the grooving. It is all very well to say, "Search the gun to see whether the charge is home and whether the bore is sound;" but how are you to do this in the heat of action, with the enemy's ship firing into you all the time? The simpler the appliances used and the more certain they are, the better. Then, with respect to the uniform spiral, what Mr. Merrifield says is perfectly correct, and he has more clearly defined its advantages than anybody I have heard. I do not wish to say anything as to faulty construction, because I feel very strongly myself that what we ought to do is to try and improve what we now have, and in new guns to have what are the very best. Therefore, if breech-loaders be the best let us have them, but let us at the same time make use of the present guns, improving them so as to become more powerful than they are at this time; first, by improving the powder; secondly, by altering the rifling to a uniform twist; and thirdly, by firing iron shot which are not rotated by studs, but by long flanges. And why by flanges? Simply because on the first impulse of the powder, the projectile, if it has long flanges, rises up from the bottom of the bore and centres itself going through the bore, clear of the dirt in the bottom. If studs be inserted instead of casting flanges on the shot, the studs yield, and the body of the shot is driven down upon the bottom of the bore, and rubs along, dragging with it the fouling, and causing an enormous pressure which has never been fairly tested, and accounting very fully for those waves of pressure, which are stated in the different reports of firing to rise from seventeen tons up to forty tons. Some tell you they do not know to what these variations are due, but it is as clear as possible that it is to the faulty system, which is likewise the cause of several Woolwich guns having broken up. I went twice to look at the 12-ton gun that burst in proof, I think at the second round, merely firing a quarter more than the service charge. How that gun burst was apparent, for there were marks of the studs having slipped over the rifling. On another occasion a 12-ton gun went to atoms at Shoeburyness. And a third 12-ton gun blew out its breech. It is therefore futile to say these guns do not break up. Undoubtedly they seldom burst, and fracture is not likely, when they have been carefully searched, but supposing there is a lengthened action, I want to know how men can search the gun and feel exactly the state it is in. We all know it is impossible. Therefore, I think we require, for the sake of economy, for the sake of the honour of our country, and for the sake of those sailors whom we send on board our ships, that there should be a full and complete investigation of the present system; and I am satisfied that if a committee or commission were appointed—not of those who have had to do with introducing the system—you would attain a satisfactory result at a very small expenditure.

Admiral SELWYN: I wish to say, my Lord, that this question has been brought forward by Mr. Merrifield at a most opportune moment, but he has touched a burning sore. We all know, notwithstanding all the things that have been adduced in favour of the present system, that we are getting gradually into worse and worse results instead of into better. We are lengthening our guns to do what? Just as the Chinese do, to burn worse powder. And why? Because the guns are weak Why are the guns weak? Because nobody would take steel when it was offered to them instead of iron. I deny altogether, both upon the question of armour-plate and upon the question of guns, that you could not have had steel as good as you could get it now, if you had asked for it ten years ago. If Mr. Bessemer had been listened to, and other men of equal eminence as himself in the manufacture of steel, and if there had not been great and undue pressure brought to bear against the use of steel, there would have been then as great an advance as we have now. But there has been an organised resistance, and why? Because there were great vested interests embarked; because to make iron was the speciality of all England, to make steel was the speciality of a few, and therefore for a long time that improvement, which promised to render useless millions of pounds worth of existing plant, was resisted, as all such improvements always are, and always will be. But the time has come when steel has asserted its own. It is too late to say that we cannot make strong guns. Guns that will burn every pound of powder and every ounce of powder that they can be charged with, have been offered to the Government for trial, and refused by the Government. They have been offered by Sir Joseph Whitworth for trial without any expense, and therefore I say we have not done so much with the question as might have been done. I say we have made the first mistake when in want of a gas to drive a projectile, in consenting to take the worst material to form that gas, because the pressure was then such as the utensils into which you put it, would bear. Instead of making the utensil stronger we made the powder-gas weaker. And it is no answer to say that if you would only keep up the pressure during a sufficiently long time, and by having a long gun, you will get an equal initial velocity. Given bad powder, you must have long guns: given weak guns, you must have bad powder. You must also have fouling, and you must also have smoke; and I do beg those who advocate breech-loading artillery for our ships to recollect what the result of firing modern charges of powder is—you have carbonic oxide gas, that yellow smoke that we know so well, proceeding from the guns which used to give pure carbonic acid alone, the result of perfect combustion. That carbonic oxide gas is fatal to human life. If you open a breech-loading gun constantly several times and let the smoke come into the turret, or casemate, as it will do if the atmospheric conditions are unfavourable, you will stifle every man in the place. These results arise from the initial error, and the right way to act is not to continue any longer, as Mr. Merrifield has said, trying to cover up defects. Let us rather examine the principles upon which we are proceeding, and go back and confess that we are wrong, if we find that to be the truth. If you have good powder in a strong steel gun, I am quite sure you can get all the advantages with a very short gun, which you now propose to derive from a very long one, combined with many other advantages which you cannot get under any other system. Upon the question of the improvement of powder I am rather at issue with Mr. Merrifield, but on all the other questions I recognise the great importance of what he has said. He has pointed out that it is not supposed to be sufficient to-day to have a gun twenty feet long, but we want to have guns thirty feet long. Now I ask this meeting whether anybody in his senses twenty years ago, when guns were first proposed to be made on this wonderful system, if he had been told that one of the conditions of adopting this system was that he must consent to have guns twice as long as they were then, would have listened to it. Has not the world been brought to consent to it gradually by successive steps, each of which has been an error? But Dr. Siemens and Sir Joseph Whitworth tell us, We can make you steel guns to any extent you please, at a cost infinitely below that of the existing built-up ones, with a strength infinitely

greater—so strong, indeed, that by no possible charge that you can put into the guns can you burst them, even if you use the quickest burning powder known. Ought it to be a question any longer that we should go back to the first principles on which we achieved a reputation many years ago, resulting from the employment of skilled artillerists, from having the very best guns, and the very best powder in the whole world, giving a muzzle velocity of 1,600 feet per second. We are slowly attaining to 1,600 feet. In what way must it be done? By confessing, not alone to the errors I have spoken of, but to the still greater one, which is, that each gun must have its own powder, or it will not do; so that you must have many natures of powder on board ship, according to the many kinds of ordnance there are on board. If you have a land service mortar on shore which is short you cannot fire ordinary pebble powder, but if you have a long gun then you must have the pebble powder. All these objections will be swept away at once when we come to consider; first, the quality of the powder; secondly, the nature of the projectile; and, thirdly, the effect of the velocity from a moderately long but very strong gun. But so long as the world will persist in piling up error upon error; in covering up a defect by introducing a compensation, which, at last, is nothing more than a second error; so long as that is done, instead of making real progress, we shall be tending towards that time when we shall have to sweep everything away, and begin afresh.

Mr. J. Scott Russell, F.R.S. (Vice-President): I am afraid we must all confess that the whole history of our naval gunnery for the last twenty years is simply a progress of error after error; an ingenious blunder created by great complication, and more ingeniously corrected by doubling the amount of complication. The great fault of our present great guns is this—that they are pieces of patchwork. They are in this respect pieces of patchwork, that instead of being a continuous tube they are little bits of tube put end on end and then tried to be connected together in some way or other, and, if I am not grievously mistaken, the explosion of this *Thunderer* gun took place merely by one bit of the gun choosing to part from another bit of the gun at a place where they had no junction. Now that this should be possible in the great guns of the British Navy is all but incomprehensible. Moreover I saw those guns, and visited those turrets, before the ship left this country, and there is nothing to my mind more alarming or horrible than that the men who are in the turret have nothing to do with the loading of the guns, and have all the work on which their lives depend done by some fellows or other down in a dark cell below them. I think it is something perfectly incredible, and it is more incredible to me that I saw also they had wobbled the deck of the ship up here and down there, merely that it might get out of the way of those fellows below and suit their convenience. Now allow me to sum up all that I think is important, and most important, in this paper of Mr. Merrifield's in a few words. Firstly, our guns are made of patches, in fragments ingeniously tied together; that must be done away with. Secondly, they had a very bad kind of gunpowder, and they had, therefore, to adopt a very ingenious French invention—namely, the accelerating groove, because they would not get a proper gunpowder and make a proper cartridge. They have now got the right gunpowder and they now know how to make a proper cartridge; but they have already rifled all the guns with the wrong rifling, and therefore they are obliged to invent this ingenious bad powder in order to suit this wonderfully ingenious bad rifling. That is the present state of the matter. And lastly, because they did invent an abominable mode of breech-loading which was dangerous and intolerable, they said, "Dear me, it will never do to say that our plan of breech-loading was abominable, that would never do. Therefore we must say the whole system of breech-loading is abominable." "Happy idea!" said the other fellow. Accordingly they said, "Breech-loading is abominable: we have tried breech-loading; you see there is a field covered with guns—breech-loaders—and not one of them is of the least use. Breech-loading is therefore no good, and we will avoid breech-loading." In other countries they have beautiful breech-loading guns which answer every purpose, and nothing but that series of blunders

which we would not honestly come forward and frankly acknowledge, has driven us into pretending that breech-loading is bad and wrong, and that we must cobble up with this underground work, or some other work, in order to avoid the blunders we have made. My Lord, I say in one word that if any Government will say to-morrow, "We want a perfect new gun without any reference to this system;" if they will say, "Give us a perfect rifling; give us a perfect structure of gun; a well-organised cartridge; give us such a bore to the gun as shall enable the gun to drive out the shot instead of bursting the gun;" then, "Let these guns be continuous from end to end, and we will not accept a bit of patchwork;"—if you will only make that request as a Government, and say that is wanted, I undertake that within twelve months the ingenuity, science, industry, and skill in steel and other matters in this country will give you perfect guns; but if you do not absolutely abandon everything you have got, then you cannot mix up anything with it without making the old muddle over again.

Mr. John R. Ravenhill: My Lord, I will not occupy the time of the meeting very long on a subject I know so little about; but it fell to my lot some few years ago to superintend the tooling up of some wrought-iron guns which were sent to our factory by a friend with a view of having them, when completed, forwarded to India. Those guns were, perhaps, some of the earliest made, and were forged solid. The difficulty with them proved to be the inability at that time to ensure soundness of material towards the centre of the forging. His idea was to introduce, if possible, the wrought iron gun in lieu of the field gun in use at that time in India. He preferred to adopt the breech-loading system. The guns were finished, and they were sent down to Woolwich, where they were proved and tried, and the result of those proof trials was against the guns. I think four went down of different sizes, and in no one case was the metal sufficiently sound and good towards the centre to stand the tests that were imposed on the gun when under the inspection of the then Woolwich authorities. But since that time changes—great changes—have taken place, and there can be no doubt that at the present moment if the authorities at Woolwich had to commence again forging large wrought iron guns, they would adopt other means than those which, according to Mr. Scott Russell, they have used in producing fragments—so many fragments equalling one gun. I think he was a little hard on the Woolwich authorities, because really at the time when these large guns were first commenced I believe the system they adopted was considered equal to anything that was then known. There can be no doubt that by hydraulic pressure, or with Sir Joseph Whitworth's fluid compressed steel, vast improvements have been made, and my own opinion is that we are on the eve of great changes, and that we shall see steel guns introduced. Now while parties are scheming for the introduction of new guns and using a new material, I will simply say to them, I advocate the breech-loading system. As a practical man I advocate it, and I would ask naval officers—and many of them I know are very, very fond, when they get ashore, of using an ordinary fowling-piece—whether they could not from just that practical day's shooting, say what a vast improvement is obtained with an ordinary breech-loading fowling-piece over the old muzzle-loader. I say to them, think over that, and I believe it is a matter which will become practicable. Give that practicability for the benefit of Jack, and you will then take one step in the future, which is this—that you will never hear of a gun bursting, or its being suggested that a gun burst, from receiving a double charge.

Mr. Michael Scott: I ask leave, my Lord, to say just one word. The conclusion which from theoretical considerations I arrived at years ago was this—that, neglecting friction and mechanical imperfections, the power to produce rotation of the shot with uniform pitch was about $1\frac{1}{2}$ per cent. of the projectile force. I will just make one remark with regard to steel, because it is a subject with which I happen to be practically acquainted. We all know what Sir Joseph Whitworth has done in connection with compressed steel, and I have no doubt he will do still more. I have reason to believe that

such pieces as hoops for guns of compressed steel could now be produced at a lower price than wrought iron rings.

Mr. C. W. Merrifield: I think, my Lord, that I have very little indeed to say, because there have been very few observations that I could reply to. I may say, with regard to the observations that fell from Mr. Macfarlane Gray, that I cannot quite agree with his theory as to permanent stress. It seems to me that the gun which he proposes is very much in the condition of first straining a rope to the greatest tension it will bear, and then putting an extra weight on it. With regard to Captain Scott's observations about the doubts as to breech-loading in Germany, it is quite true that I have heard those doubts expressed, but I rather think it is with the older natures of breech-loaders used in Germany than with the more recent guns. At any rate, I find Prussian officers are not so dissatisfied with their guns as to propose in any way to revert to muzzle-loading. With regard to what fell from Admiral Selwyn as to the dangerous gas getting out more seriously in breech-loading, I have no doubt that is the case where bad powder is burnt, and I suppose, under any circumstances, if the breech of a gun is to be thrown open, especially with the wind blowing down the muzzle, in a confined turret, some means would have to be taken, so that the gas would blow out of the gun in some other way, instead of back into the turret. That presents no mechanical difficulty; it merely wants a fan, or a blower of some sort, which involves no complication whatever. I think these are all the remarks that I have to make. To tell the truth, I am disappointed to find that I have so little to reply to. I was in hopes there would have been a little opposition to my views; but, as it is, I can only thank the meeting for the attention they have given me.

The President: I am sure we shall all thank Mr. Merrifield for having contributed a short paper but a very valuable paper—a very well written paper, on a subject which at this moment, always indeed, but especially now, is one of the greatest possible moment. I have listened with great interest to the discussion that it has led to; I regret, with Mr. Merrifield, that the discussion has not been longer, because we cannot hear too much of such an important subject. I am sorry to say that my engagements will not allow me to attend the evening meeting; but my friend, Mr. Barnaby, one of the Vice-Presidents, has been kind enough to say he will take the chair.

ON THE TRUE NATURE OF THE WAVE OF TRANSLATION, AND THE PART IT PLAYS IN REMOVING THE WATER OUT OF THE WAY OF A SHIP WITH LEAST RESISTANCE.

By J. Scott Russell, F.R.S., Vice-President.

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, N. Barnaby, Esq., Vice-President, in the Chair.]

The Chairman: Gentlemen, we are to have the pleasure of receiving from Mr. Scott Russell this evening the first part of the paper on "The Wave of Translation." The second part of the paper will, we hope, be given to-morrow evening.* It is not proposed to take any discussion on Mr. Scott Russell's paper this evening, and we hope to be able, after he has read the first part, to take some papers of the gentlemen who are named here, on "Screw Propulsion." You heard this morning that of the papers which were sent in for competition for the Carlyle Gold Medal, Mr. Maginnis had prepared that which was considered by the Council to be the best, and the Gold Medal has been awarded to him. The papers appearing here in the names of Mr. Maginnis, Mr. Griffiths, Mr. Riley, and the Honourable Mr. Parsons, have also been considered by the Council to be good papers which might be read, provided we have the time for reading them; but that will remain to be seen.

Mr. Scott Russell: The "wave lines" of a ship, and the "wave principle" of least resistance, are already so well known to naval constructors, that it had for many years seemed to me unnecessary to trouble the members of my profession with any further explanation on my part, either of the nature of the wave which removes the water out of the way of a ship, or of the nature and measure of the work it does in the act of removal. But a paper read at this Institution last year, and some papers printed elsewhere, have shown that it is still possible to misunderstand the nature of the wave of the first order; to misconceive the function it performs in the removal of the water, and therefore to misapply it, by using mis-shapen forms of so-called wave lines. I therefore feel it a duty, for which I am responsible, not to leave any important part of the fundamental truths of wave line construction, or wave nature, either ambiguous or obscure, and I must express my regret that my former explanations should have left it possible that they could either be misunderstood, misrepresented, or misapplied.

In the process of removal of water out of the way of a ship two actions go on together. One is "what the ship does to the water:" this is confined to the actual contact of the surface of the ship with the water it forces away. The other is "what the particles of water do to the other particles of water," none of which are touched by the ship. The nature of the blow with which the ship strikes the water, that is the shipbuilder's doing, that is his free choice. The nature of the movement the water will take, what direction it will select,

* For the convenience of the reader, both parts of Mr. Scott Russell's paper are placed together.

what speed it will keep, what shape it will assume, that is a matter over which the ship-builder and his ship have no control. The blow once delivered to the water, all its consequences depend on the choice of the particles of the water, as to what each particle will choose to do, each and all acting in harmony with each other. Now it appears to be the thought of some of the able writers on this subject, that if the blows delivered by the ship, to the water it strikes, are given in the right quantity, the quality of the blow matters little, the direction in which the water particles are hit matters little, the speed it is hit matters less; what is wanted is a given blow of a given quantity. They think, and say, that all the rest will be settled among the particles themselves, and need not concern the ship or its builder. This I shall show you to be a fatal error, in thought, in fact, and in practical work. It is an error of old tradition, disproved by fact of experience, and tests of experiment over and over again, but it is a natural thought, a seductive error, and its adoption is difficult to resist. The nature of this error is as follows:—

Take two ships' fore bodies alike in length, breadth, and bulk, alike in size of midship section, differing only in shape of line; let the one have water lines like the wave form, the other water lines like a fish; then, say they, so long as the bulks in water at like distance from the bow are alike, the one shape is as good as the other, the resulting work done by the ships will be alike. (See Figs. 12, 13, Pl. IV.)

What is to be said in this case is, that the mass to be removed is in both alike, and that the manner of the removal is quite different. What is not true is, that the manner of removal does not matter, so long as the bulks of matter to be moved in equal times are alike. I have, therefore, now to show the important truth that the manner of the removal is of the essence of the result at which we aim—namely, how to remove the given mass of water out of the way of the ship with the least possible resistance.

For this purpose, I say—

We must remove it to the right place.
We must take it along the right way.
We must place it at the right time.

These are the essence of our aim and the conditions of our work, which cannot be neglected.

I.

Thus we have three problems to work out—

1. Where shall we place the removed water?

We must place it where there is room. Is that room below the water? No, is the answer, below is full. Is there not room on either side? No, all round about is already full. The answer then is, Nowhere under water is there any room; all is full.

Now the essence of the proposed work for this erroneous form of ship must be to remove the water downwards and outwards into the space where there is no room left for more.

Our conclusion, therefore, is that the only place into which the water in the way of a

ship can be removed, must be into the space left free, and that space is above the level, not below.

II.

The second problem is—

Along which path shall we remove the water in our way out into the free space left for it, on the top of the water level?

To think out this, let us take a smooth basin of still water. Let us take an instrument —say a spade, an oar, or any thin flat plane—through which we can get the water out of the way. Next, let us ask, which is the nearest way to the top of the water level? The answer is straight upwards, or square to the water's upper face.

If we simply lift or raise the hindering matter right up out of the way, and lay it on the top of the water, while our ship takes the vacant place below, that will do what we want, and that will be the shortest way.

Straight up to the top of the water is the shortest way to take the obstructive water into free space.

III.

The third problem is—At what time shall we take the water up there, and how shall we lay it out on the surface so as to offer little or least obstruction by being up there all around?

Now it is plain that if each portion of water, as sent upwards, were left just above where it was found, only on the surface, this water once moved might have to be removed a second time. This gathering of removed water is a cause of great hindrance in ill-shapen ships.

We must therefore make a serious study of the time and place of removal, and the wise means of effectually doing it.

The wave principle shows how to time the removals, and the wave form of ship gives us the tool to do it.

The matters discussed in this paper are therefore twofold—the Nature of the Ship, and the Needs of the Ship-builder; and the Nature of the Wave, and the Work it does in removing the Water out of the Way of the Ship.

Part I.

The Nature of the Ship, and the Needs of the Ship-builder.

It is the purpose of a ship to carry valuable cargo from place to place with least waste of time, work, and money.

It is the duty of the ship-builder to plan and build his ship so that it shall fulfil the purpose of the owner, and do his work with least loss of time and least expenditure of power and money.

Now, in all ships, and especially in steam-ships, the propelling power is a principal element of cost. Large sail power means large sails, masts, and yards, and large crew. Large horse-power means large room for boilers, engines, and fuel, and each ton of room taken from cargo and given to fuel involves double loss—cargo kept out and coal spent.

Great propelling power, large steam-machinery, great coal stores, are the great sources of cost and causes of waste in modern commerce on the ocean, and the matters we have to examine are—How we can economise the resources of the ship-owner, in better engines, better boilers, better ships.

How to economise the power which propels the ship by size, proportion, and shape of ship is the special problem before us as ship-builders; and that problem, put in other words, is, to give the ship that shape which will go through the water at the wanted speed with least waste of power. This shape we are accustomed to call the Form of Least Resistance. This form Sir Isaac Newton took to be the paraboloid. I have ventured to propose the wave-line as more perfect for ships. I propose on this occasion to show, by proceeding from established axioms of physics, by incontestable principles, and by geometric method, that the wave form of ship enables the power propelling that ship to go forward through water on the surface of the sea, so as to experience least resistance from the water outside, and to waste least of the propelling power inside.

I. Axiom.—Change of place with least resistance implies—using least moving force, following the shortest path, removing the least mass to the least distance.

II. Axiom.—Waste of power means—moving more mass than necessary, giving it more speed than necessary, carrying it farther than necessary.

III. Problem.—Thus the problem becomes—How to take a given mass (of water) out of one place (where it rests), and carry it on to another place, with given speed, and to leave it in that new place perfectly at rest.

To carry it farther or faster than needed, or to part from it while still in motion, is so much power wasted.

IV. Principle.—To move a given mass of matter out of rest into motion by least force implies a constant force and accelerating velocity like that of gravity. To restore the moving mass to rest requires retarding constant force, diminishing that velocity to nothing.

V. Experiment.—Take Attwood's Gravitation Apparatus. A mass of matter, parted in halves, joined by a thread, hung over a wheel, and at rest, the whole mass weighing 2 lbs.

Take a moving force of two ounces (a 2 oz. weight), and lay it on one side for one second of time, it will move the whole mass one foot (nearly). Take it off and place it (at once) on the other side, and leave it there; it will move the contrary way, and will be found at rest in the new place at the end of another second of time. (See Fig. 1, Pl. III.)

VI. Conclusion.—The whole of the moving force stored in the two ounces was used in the first phase of motion in producing the due speed. The force thus spent carries the whole mass through a second foot in the same direction, and is thus utilised to carry the mass along a double distance, while it at the same time restores the 2 oz. force to its original place.

Here we have an example of the simplest manner in which a moving force may be applied to remove a mass in repose from one place and carry it by a uniform force with accelerating speed to another place, and then to carry it with retarding speed and uniform force to a further place twice as far off, and deposit it there at rest.

In this process there is nearly no waste and no loss of power, and the work done cannot

be done by less power or with less waste, and therefore may be said to be done with no loss and no waste, or at minimum cost.

VII.—Application.—This leads us to the case of the ship, and her action on the water. The ship, by help of her driving power, has to remove the water out of her way. The ship-owner wishes this done with the least waste. The ship-builder has to find how to do it.

Let us consider this matter in its simplest form. A mass of matter is lying at rest in the way of the ship. It has to be moved out. The questions which arise are—Which way shall we take it out? and, Where shall we lay it? Thus come the questions:

Questions—Which is the shortest way?
Which is the convenient place?
What time have we for the work?

Take a surface of still water.

Take a water-way to be cleared, 6 ft. deep and 12 ft. wide, one half on the right of the stem, the other left.

Here, then, we see that for each foot of the ship's way forward a mass of 36 cubic feet, weighing 36 by 64 lbs., has to be cleared out, lifted up, and laid out above the water level. That is the work to be done on the two sides of the stem.

Question I.—Which is the Shortest Way? (See Fig. 2, Pl. III.)

It is plain that the shortest way out is the line perpendicular to the surface level, the upright path. Also it is plain that all other ways are already occupied by other water, which we have no need to move. The only free space left to us is the vacant room above the water, and every other way is hindered by a longer column of water already occupying it. This is the shortest way out, and it is the only free way.

Question II.—Which is the Convenient Place? (See Figs. 3 and 5, Pl. III.)

The convenient place must be either that which is nearest to hand or that which requires least effort to place it there. Now both of these depend on the "manner of distribution" of the mass when removed. If the mass be piled up to a great height the distance it must be raised above the level will be "added to" the distance raised from below, therefore this height of removed mass above the level should be minimised. If the whole mass in the ship's way were lifted above the level without change of shape, its centre of gravity would be raised twice as high as its depth in repose below the level. This excess has to be minimised. The way to minimise this excess is "to contrive how to spread out the displaced mass over the largest area," so as to reduce the added height to a minimum.

Question III.—What Time have we in which this work may be done?

The time given is involved in the speed wanted for the ship, and the length allowed for the entrance. If the speed wanted were ten miles an hour, or 16 ft. a second, and the length of ship's entrance were 32 ft., all this work of removal and distribution would have to be completed in two seconds of time; but if the length of entrance allowed were 64 ft., there would be four seconds of time allowed for it; and with an entrance of 128 ft. long eight seconds would be the time given for the work.

VIII.—Mechanism of Execution.—We have now got the problem clearly defined. The

water in the ship's way must be raised up vertically to give the shortest way out. It must when raised up be spread wide out in order to diminish the vertical height to a minimum. The maximum length of time must be given for the work by length of ship's entrance, or length of bow. (See Fig. 14, Pl. IV.)

The wave-line bow is the form which generates the perfect wave of the first order. This wave raises the water below the level in a vertical column upwards along the shortest way out, and, without further cost, spreads it horizontally over a wide wave area, at a small height, and the wave line gives the necessary length of time to do the work without waste.

IX.—The manner in which the Wave-line Bow generates its Wave of Translation is as follows:— (See Fig. 15, Pl. IV.)

The upright stem, made fine and thin, may be considered as a vertical plane at right angles to the cross section of the ship, and having motion given to it outwards from the central line of keel. The motion of the ship forwards, introducing a wider part of the ship, gently moves this vertical plane outwards, and gently moves the water outward with it. If the time given were eight seconds for the length of bow, and 100 inches for half the beam of the ship, then in each half second the vertical plane would push the water aside through the following spaces in successive times:—

Half seconds—1, 2, 3, 4, 5, 6, 7, 8; 9, 10, 11, 12, 13, 14, 15, 16.
Removals—1, 3, 5, 7, 7, 8, 9, 10; 10, 9, 8, 7, 7, 5, 3, 1.
Total Times—$\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, $3\frac{1}{2}$, 4; $4\frac{1}{2}$, 5, $5\frac{1}{2}$, 6, $6\frac{1}{2}$, 7, $7\frac{1}{2}$, 8.
Total Removals—1, 4, 9, 16, 23, 31, 40, 50; 60, 69, 77, 84, 91, 96, 99, 100.

Thus the vertical side of the bow becomes water-mover, and the generator of the wave of translation.

These successive removals by the bow generate at each instant the heights due to the velocities of removal, and these heights are the isochronous heights of the wave of translation. (See Figs. 6 and 7, Pl. III.)

Thus the work the ship does is to generate the wave of translation, and what the wave of translation does is to carry off the moved volume of water horizontally and laterally out of the way.

For all that follows we must seek our knowledge from the study of the nature and laws of the water motions in the wave of translation. And we must look there also for the influence of depth of the sea on the dimensions and velocity of the wave of translation.

Part II.

The Nature of the Wave, and the Work it does in removing the Water out of the Way of the Ship.

1.—How can the wave of translation remove a mass of still water out of one place, and lay it in a new place, and leave it there in a state of perfect repose?

2.—How can a wave of translation be created by a ship so as to do this work with least waste?

The genesis of a wave of translation without waste can be done as follows :—

Given—A sheet of still water.
A thin, flat, smooth, strong board.
A power to move this board.

Place this board upright in the water.
Push the board forward 1 inch.
Push it 3 inches more, then 5 inches, then 7.
Push it further, 7, 8, 9, and 10 inches.
All at one rate.
Each movement in the same time. (See Diagram XI. Pl. IV.)
Now slacken the speed—
Move on 10, 9, 8, and 7 inches,
Next 7, 5, 3, and 1 inch,
Coming to Rest. · Stop.
You have removed the water.
You have sent it to a new place.
The water rests in a new place.
You have created the wave. (See Diagram IX. Pl. IV.)
But the wave created flies onward afar.

The wave genesis is finished.
The water is removed out of the former place and is left at rest in its new place.
This double process of genesis consists—
In gradual acceleration, followed by gradual retardation, producing—

1. Growing speed.
2. Maximum speed.
3. Diminishing speed.
4. Repose. In translation. Completed,

The complete translation consists of 100 inches of forward removal of the whole mass of water pushed into the new place, and left there. (See Diagram X. Pl. IV.)

This removal completed, and the mass deposited, what are its consequences?

[1] The mass removed has found a new place.

[2] This new place was occupied by another mass.

[3] This displaced mass [2] must in turn have a new place.

[4] This new place is now occupied by mass [3]. Therefore [3] mass must go to [4] place.

In place and out of place must therefore go on as follows :—

[1] shifted into [2], [2] into [3], [3] into [4], [4] into 5, 5 into 6, and so on endless.

In successive times these displacements will take the following phases :—

		1			
1	2,				
			2		
0	1	3,			
				3	
0	1	2	4,		
					4
0	1	2	3	5,	
					5
0	1	2	3	4	6,

And so on endless.

There is thus presented to our thought an endless series of masses taken from their place, raised up above their level, carried forward, settled into a new place, whence another mass has been already displaced, raised above the level, and also set down in a new place; and so on, each after each, endless.

This series of consequences of a single act of moving a single mass forms a succession of phenomena endless in number, endless in time, continuing ever onwards one way.

But because this countless series of changes continues going forward changeless the same way in endless repetition, we have given one name to the whole endless series, we call them all together "a wave." One wave—as if it were only one mass of matter travelling onwards. Whereas it is nothing of the kind, but is thought of by us as if it were one thing, keeping shape while changing place.

What we have to keep in mind is the perpetual change of substance and matter, while the shape and bulk are unchanged, and only the place filled, constantly going forward one way, the seeming unity being merely endless, changeless change.

Having now effected by a simple forward movement of a flat plate, in a regulated manner, the displacement of a given mass, in such manner as to be followed by an endless number of like displacements flowing from it, as effects from cause, in like manner and endless uniformity of mass and bulk, we may now take a new step in thought and fact.

What is the nature of the mechanism by which our single phase of removing one mass from one place is repeated over many places and continued onward?

Wave Mechanism.

Ths question may be put in another equivalent form.

What does each atom of water endure while the wave is being created? And what does each atom do aftrewards while it takes its part in continuing and repeating the process of

leaving one place, driving another atom out of its place, and assuming for itself the place thus taken?

The answer is neither simple nor easy. We must treat this wave motion in due analogy with other known mechanical motions. (See Fig. 8, Pl. III.)

To represent time, take the round face of a clock. The circle of the second-hand may suit us best.

To represent the raising of an atom of water up or letting it down, take the length of the second-hand of the clock as measure of height.

Between those two, the times shown around the clock face, and the height fallen by an atom, and measured by the hand of the clock, there is a wondrous and close bond.

When I take an atom of water, raise it above the atoms next to it, and then let it fall gently down, it gently raises its neighbour to the height whence it falls and leaves it there, while itself remains at rest where it has fallen.

The fall of the one atom takes time. The rise of the other atom takes time. The question of time we must seek from "the round of our clock."

Let ten inches be agreed as the fall of our atom, and ten inches be given as the face of our clock.

If we now set the clock face and falling atom side by side; if we mark the place of the falling atom and the falling hand of the clock, and also mark the place of the rising atom and the rising hand of the clock;

We shall find the following relations of place and time; the ascending hand going round falls; and just so far does the descending atom going straight fall. The ascending hand going round rises; just as much does the ascending atom going straight rise.

Thus we time each atom going down and each atom going up exactly by the hands of the clock, and the measure of the fall and rise of the atom is identical with the rise and fall of the opposite ends of the hand of the clock at the same time.

If as an example we take an atom falling ten inches, and the time of descent—six instants—the fall will be as follows:—

Fall. Inches.			Time. Instants.	Rise. Inches.			Time. Instants.
0·0	....	....	0·0	10·0	....	....	6
1·3	....	....	1	8·7	....	....	5
2·5	....	....	2	7·5	....	....	4
5·0	....	....	3	5·0	....	....	3
7·5	....	....	4	2·5	....	....	2
8·7	....	....	5	1·3	....	....	1
10·0	....	....	6	0·0	....	....	0

And each falling atom has its match in a companion rising atom.

We have now found a way of moving an atom of water so as to connect together its change of place with the flow of time, in our own thought. The question now presses on

us, can we create this regular atom movement by any movements or mechanism under our own control? We must seek, contrive, or construct such mechanism.

The mechanism we seek is a mechanism which shall produce certain desired movements of rise and fall, in given equal intervals of time, among atoms of still water.

Take a few smooth thin sheets of glass (or ice?) and arrange them in order, upright, equidistant, in one sheet of water, parting the atoms of water in their vertical columns, side by side.

Squeeze the first plate towards the second plate, so as to raise the column of water between, ten inches above the level of the rest, and leave it there.

Suddenly raise the second plate from the bottom so as to let the water between I. and II. sink to the level. As it falls, so must the water between II. and III. rise. Then press down plate II. into its place again, and mark the result.

The first water atom has gone down ten inches. The second water atom has gone up ten inches, and both are at rest, each in a new place.

But it might be said, the one column going down will raise the other half-way up when itself is half-way down. It will so. They will then be on the same level, half-way up and half-way down, and they may both stay there at rest. They both would stay resting there if they were slowly let down with that intent. But in fact, just then both are in full swing, each its own way, and neither can stop itself nor hinder its neighbour till both have made their full swing. This swing ended, leaves I. on the level, II. at full height.

We have now seen how by suitable mechanism three vertical glass plates, dividing two vertical columns of still water, may be so used, that plate I. raising one column to given height and stopping, plate II. being raised, allows that column gently to drop; and while dropping to push a second column gently upwards; first to half the height where the two meet, each in full swing, one down and one up; and at last one column at the bottom, and the other at the top. Then plate II. is closed, and the result is:—the first column has gone back to the first level; the second column has gone up to the full height from which the first has gone down.

The first column has been raised, descended, and returned to rest. The second column has been raised to the height from which the first fell, and stays there. Such is the first complete operation ended.

A second operation may now begin.

Between plate II. and plate IV. stands plate III., dividing column second from third. The second column is raised, the third column is on the level.

Now open the communication betwixt them by raising plate III. Allow second column to descend with the clock, 1·3, 2·5, 5·0, 7·5, 8·7, ten inches, thus compelling the third column to ascend in the same times by the clock, 1·3, 2·5, 5·0, 7·5, 8·7, ten inches. Now shut off all communication by putting down plate III. into its place again.

The results are:—columns one and two now on the original level. Column three at the full height.

Thus a second complete operation has been performed, one atom of water descending,

and thereby raising another atom in another place to the full height by successive stages, meeting midway, and passing contrariwise, while the hands of the clock go a complete round, making like ascents and descents, and in the end placing the third atom at the same height above the level at which the first atom originally stood.

But a third operation of like sort may now be begun.

Column three now stands at the full height of the original first column. It stands between plate III. and plate IV., but there is a fourth column left between plate IV. and V. on the level.

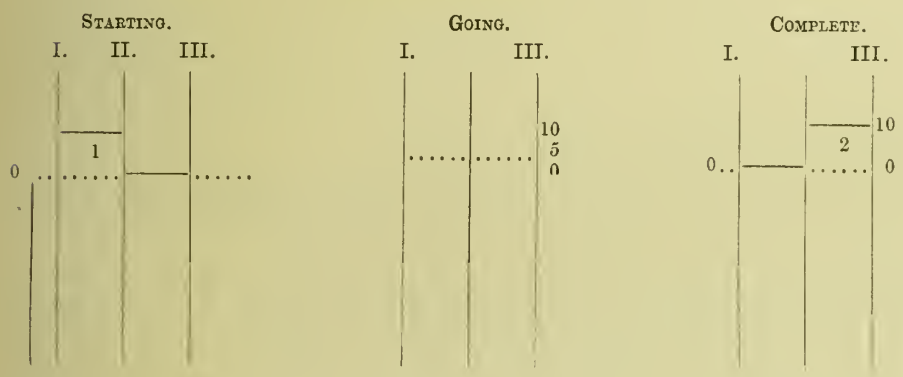
Let us now raise the dividing plate IV. Column three descends, raises column four, they meet at half height, in full swing. Column three completes its descent; column four completes its ascent; shut down plate IV. Column four rests now at full height. Column three has returned to rest on the original level.

On the way down atom $\boxed{3}$ fell 1·3, 2·5, 5, 7·5, 8·7, ten inches.

On the way up atom $\boxed{4}$ rose 1·3, 2·5, 5, 7·5, 8·7, ten inches.

Meanwhile the hand of the clock pointed equal instants of time, and doing so, rose and fell equal heights above the level.

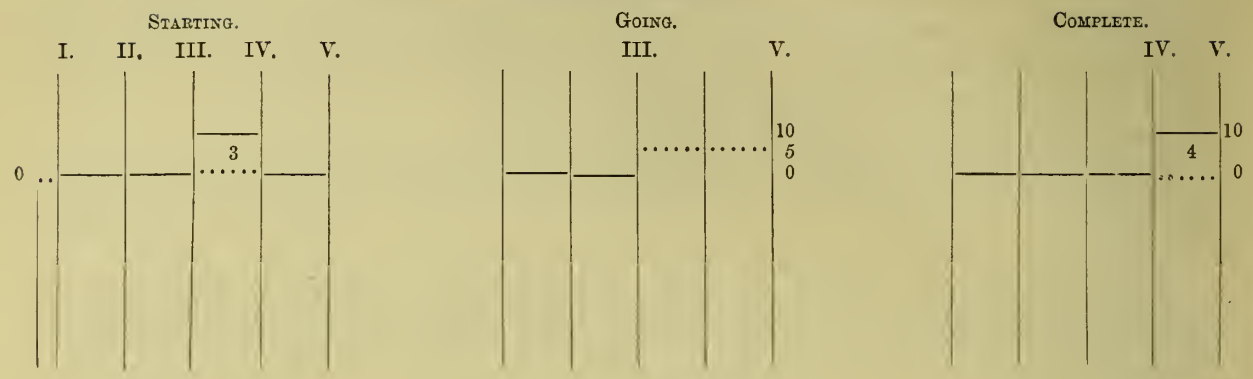
The series of like operations may go on likewise, endless.



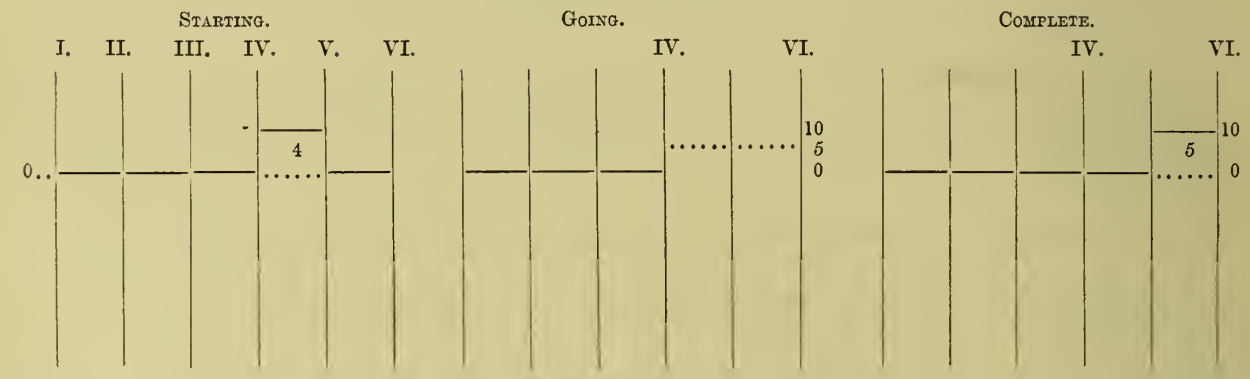
First Operation.

Second Operation.

THIRD OPERATION.



FOURTH OPERATION.



Taken on a larger scale the rise and fall is given in detail.

Near. 0 at starting, 0, 4, 15, 30, 45, 56, 60, ascending.—Ending, 60, 56, 45, 30, 15, 4, 0, descending.

Heretofore, we have only been thinking out and contriving how an atom of matter floating in a liquid mass might be set up in a new place; how it might be so guided in descending from that new place as to send up a second atom to another high place; how the first atom might be left at rest in its own new place while the second atom was in turn displacing a third atom in descending to rest in its own resting-place; and how this third atom when raised up into its new place should in turn descend to its resting-place, while raising a fourth atom up into its higher place; and so on in order a fifth, sixth, and continuing line of atoms, should go on ascending and displacing each by each, and descending and replacing each by the other, in unending line and order.

We have seen how atoms afloat in liquid at rest might be so guided and moved as to make these changes of place in orderly following; by regulated change of form and place without change of bulk or space. This as mere manipulation of space and place is scarcely more than mere geometry of place occupied by mass, without hindrance to change of shape, but without a thought of change of bulk.

But we have now to seek in nature and matter, and find out whether there exist in nature some force to cause such movements as we have contrived and controlled and carried on? whether such force would do its work in the time we want? whether we could safely trust it freely to do our work without our supervision or interference.

Geometric Representation.

Happily for us, the way and speed and time of each force, when combined in one atom, are thinkable in a new form, represented by one new line and one new force in the old time but in a new speed. This new mode of thought, this new way of matter, this new result of twin forces, is to the other two as the two square crossing lines are to their skew line. This third line is the type in way and length of the direction, speed, and strength of a third movement and of a third force—the type of the first two.

Here geometry, motion, and time agree, and likewise way, time, speed, and matter.

From these simple elements—time, force, and space, uniting in speed, way, and motion—we can now proceed to more complex forms of the works of force with time.

There exists a happy harmony of the geometric nature of the circle—with this elementary view of simple force, its way, speed, and work—which enables us to solve problems by their union in thought, which are less easily reached by other ways. (See Diagram I. Pl. II.)

This harmony I may express thus: That the harmonics of lines in a circle represent harmonics of space, force, and time.

If a point in the circle represent the place of an atom in space; if the diameter of the circle represent the measure of that ruling force; chords from the atom to the extremities of the diameter represent the elementary forces, the arcs of the circle represent the time of the elementary motions, and the lengths of the chords their speeds.

Now, this harmony of space, force, and time is true not merely of one single place and one atom in a circle, but of all the points and atoms set all round that circle; and also of all the points of all atoms all round a series of other circles. It is therefore well worth while to master the hidden nature of this pregnant harmony.

The circle expresses the harmonic relation of time with space and force owing to the following cause.

We have already seen that two simple forces right across each, do their own work without interference one with the other.

Imagine in a circle one diametral force—say vertical. (See Diagram II. Pl. II.) Imagine a directing line going out from either extremity. Imagine the atom to be moved along that line.

Then: This diametral force—acting on a mass—controlled by the directing line, will produce a uniformly accelerated motion, slower as the direction is more and more oblique to the diametral line.

Now, the remarkable property of these oblique lines in the circle is, that all the pairs

of lines on one side of the diameter meet at right angles, and therefore each one of the pair represents a force line and a directing line.

And therefore: All the motions along each line of resolved force will be each of different speed, and all performed in and at the same time.

This is the case of perfect harmonic isochronism.

Thus one and the same force produces many simultaneous motions—all of varied lengths and speed—all in one time, all in different directions.

The cause of this isochronism is that the resolved part of the force along each directing line is greater and less, exactly in the same degree as the line to be described within the circle is greater or less; that is, the greater distance has more force, and the less distance less force acting on the mass moved along it, and so the fast and slow, the long and short keep exactly one time.

From this also follows the beautiful conclusion, that any number of atoms issuing from the extremity of this diameter at either end [pole?] at the same instant, will all move away preserving their places in the same simultaneous arcs of successive circles. This may be inverted from the opposite pole. This leads to a beautiful experiment with ivory balls.

Thus: We have got harmonic synchronism from varied force with varied space.

Next we have to seek harmonic succession, or time-keeping melody.

Heretofore we have had each force remaining through time the same—each in settled proportion, and all agreeing in the same circle of form.

But the times and speeds of each portion of each path have varied far from uniformity, and have obeyed the law of constant force and uniform acceleration.

What is first noteworthy is that though the speeds and lengths in each force line differ, yet in the successive intervals of time all described the chords of circles which part them all into like proportion.

Thus not only are all the completed lines of way ended at the same instant, but all their proportional parts are finished at the same instant in synchronous succession.

Heretofore we have taken one diametral force and resolved it separately into various directing lines, and treated each resolved force and its directing line as separate, independent phenomena.

But if I could contrive that this diametral force should continue one, and only vary its direction successfully into each of its other lines at successive instants, then a proportional part of each path could be gone through in each instant, all in one line.

The cycloid is a curve in which these successive chords become the radii of a continuous curve, and the curve represents a succession of such paths parallel to these chords, and the lengths of the arcs of these curves vary in the required proportion, *i.e.*, as the forces which are directed to the given point, and this curve is isochronous.

Hydrodynamic Isochronism.

If, therefore, we make a directing line which is a cycloid, instead of a chord, all the arcs of this cycloid will have the property of these chords, and a ball will descend

along that line with like descending force to that in the chords, and will possess like isochronism.

From the gravity of atoms moving along rigid directing lines we have got isochronism, by giving a fixed length of path for a given degree of acceleration. We have now to ask how such action can be contrived in atoms flowing free (in fluids)?

The answer is that the mechanism of isochronism is already provided in the nature of free fluids.

A vertical column of water forced above the level, between two plates, has already this quality in its own nature. Its downward force is precisely measured by its height above the surrounding level. If, therefore, a vertical column descend freely through its own height above the level, it might be expected to make that descent from a greater height with greater force in the same time as a descent from a less height with less force in that time.

We have now to see what is the nature of the conditions in water which will render this possible, because in water no column can fall without its change of place affecting the particles around.

Our three vertical plates enable us to study this question. Let us take plates I. and II. in enclosed water, let us bring them together at equal distance by pushing I. towards II., raise the water between. Let us then insert III. at like distance from II., pull up II., let the water descend till it meets the other water rising, and stop them there: that takes a given time. Next repeat the experiment, and let the water descend to the level *outside;* that will take twice as long, and will leave the second up at the full height of the first: stop it there. Twice the space is now passed through in twice the time; call the one a half swing, the other a whole swing. (See Figures, pages 69, 70.)

Next raise the water between I. and II. twice as high as before, and let it perform the half fall and the whole fall; it will be done in the same time through the larger fall and through the less fall, through the half of the larger and the half of the less.

Then double the height over again, the fall will still be done in the same time.

Our vertical falling fluid column has given us the same isochronism we got in the sloping fall of the atom in the circular chord, and this will continue till we exceed certain limits.

The cause we have found is this, that the hydraulic pressure of a liquid column increases in exact proportion to the height of that column above the adjacent surface, and that if the pressure of that column is so applied as only to move a given weight of water through a space proportioned to that pressure, the longer and the shorter spaces will be performed under larger and less forces, and so in equal times.

Our three vertical plates and two vertical columns have given us isochronism.

But this isochronism of liquid columns differs from the isochronism of atoms in circular chords, by the fact that though both make like descents and ascents, the one goes up and down along slopes, while the others go up and down in vertical columns. (See Diagram III. Pl. II.)

Fortunately this seeming discord is reconciled by a new quality of these sloping paths in the circle. That they are repeated in exact identity by vertical paths in a circle, provided that circle be of double the diameter. The same lines that were sloping are now vertical.

Having thus obtained the conditions of isochronism in the relations of force in vertical lines or vertical columns of fluid, viz., that these water columns must have the same heights above a common level, which the chords of our circle have of distance from the bottom of the diameter, and having found the geometrical relation that these sloping chords are identical in length with vertical sines of arcs of the same length, in a semicircle of double the diameter, we have only to reconcile this geometrical relation with the conditions of our fluid columns, to have complete isochronism of fall of each and all those water columns.

Also, as the conditions of the forces are the same, what is true in one case of all the successive parts of fall in the circle, will be true in the semicircle of fluid, viz., complete isochronism of the parts of the fall in like differences of height. (See Diagram IV. Pl. II.)

Just, therefore, as we showed before the synchronous places of each particle in the sloping circle, can we now mark off like synchronous places in the vertical semicircle.

Having thus provided the conditions necessary to obtain perfect equality in the times of descent or ascent of liquid atoms, by forming them into vertical columns in free space, we have next to seek out, not the nature of the descents which will all agree in one and the same time, but the degrees of ascent and descent which will take place in measures of time. We do not want agreements given of one time, we want the differences of place at different successive times.

Now there is a fortunate coincidence of a quality of certain lines in the circle with the law of force acting on vertical columns, which gives us the harmony we seek, without discord with what we have already done.

We want to know the steps of ascent and descent of our vertical columns during each successive instant of time.

The answer is this: Take the original circle with its arcs to represent successive instants of time. Take the original chords of these arcs, which represent our vertical forces in lengths; then draw the horizontal sines of these arcs which cut the vertical diameter into corresponding parts.

Now the chords represent the lengths of our vertical columns, and they also represent the relative *velocities* of the motions along each chord. But they do not show the places of the particles during the motion, nor measure the path which in each case has been described. (See Diagram II. Pl. II.)

But the corresponding divisions of the diameter are exactly proportioned to squares of the chords, and as the chords measure the speeds, their squares measure the spaces described by the growing and diminishing spaces described with those speeds.

Take, therefore, in like manner, a series of vertical columns, the force in each in the ratio of the distance from the equilibrium point, and ask what length of path each atom will describe in successive equal times, and the answer will be given by the divisions we have just made on the vertical diameter of the circle.

As example take our pair of descending and ascending water columns. The mass to be moved is their sum. The moving force is their difference of height. The original level of the water is the resting-place of one and the starting-place of the other; they meet half-way, where both change from acceleration to retardation. (See Diagrams V. and VI. Pl. II.)

The spaces described in the successive instants of rise and fall, are those divisions of the diameter which correspond to the squares of the speeds, and are shown by the *versed sines* of the arcs of our chords of isochronism.

The geometry of the circle has therefore taught us—

1. The isochronism of motions produced by uniform forces directed through chords of arcs corresponding to times.
2. The isochronism of corresponding vertical motions in columns having like distribution of force in the sines of double the former arcs.
3. Place and distance done by each particle in its own path at the successive instants of time represented by the arcs, whose versed sines give the heights risen and depths fallen by each particle in those times.

Horizontal Transfer of Force.

We can now take a couple of water columns, and predict of them the isochronism of their ascents and descents large and small. We can also show the place each particle will occupy in successive instants of time.

But we have not yet considered a new phenomenon, arising inevitably from the motion of a liquid column, which has been confined exclusively to a vertical direction, but which we next proceed to release from confinement and leave free to take horizontal motion simultaneously with its vertical motion.

To find out the nature of the simultaneous horizontal motions that may take place while the vertical motions follow the law we have learned, I will make a slight but important change in the wave-making apparatus I have used or imagined heretofore. (See Figs., pages 69, 70.)

I began with a vertical plate I., with which I raised a vertical column between it and plate II. above the surrounding level; I next raised plate II. and let the first column fall vertically down so as to raise the second column between plates II. and III.

I now propose a new way of transfer of force from the first to the second column. I propose instead of raising up my smooth glass (or ice) partition wall II., to leave it for an instant "free." Now this wall II. is pressed *forward* all over its underwater surface by the hydraulic pressure due to the height of the first column above the level. Plate II. thus pressed moves forward, it removes column two forward and upward: column one has gone down and forward. Column two has been removed a like degree upward and forward.

Stopping the motion of plate II. we find column one gone down to the level. We find its equivalent in water gone forward, we find an equivalent of water in column two displaced by column one, and we find that equivalent replaced at the top of column two, and there

also the equivalent of hydraulic force spent from the top of column one, we now find re-stored at the top of column two.

We have thus from one—descent, forward motion, displacement, and power spent.

We have thus in two—displacement endured, forward motion forced, water column raised up, power re-stored.

Thus by leaving the motions of these adjacent twin columns free, forward as well as up and down, I have obtained the same ascent and descent as before, but by unrestrained forward motion under free hydraulic force.

Also, we are now ready to free plate III. from restraint, and let it move forward from the stored force of column two, also horizontally; and so raising to the top of column three its reservoir of stored force.

Thus I can, by removing impediment after impediment out of the way, transfer column after column of stored force horizontally, from place to place without limit of distance, time, or number.

The limit of time is my dexterity, or my discretion, and we must next seek how to get rid of these impediments and see how we may leave this to nature.

Let us ask how nature would carry on this work if once set going and left to herself.

What is the exact work we leave to nature? (See Diagram VIII. Pl. II.)

We have stored some force in a hydraulic column above the level, which we want to be used and transferred without loss from place to place endless.

For this purpose we have an initial reservoir or series of reservoirs; we have a sheet of water of given depth, at rest; we deliver a portion of our stored force to the transmitting water. Let us see what will follow—as we have already seen, this stored water descends and takes its place under the level, whence it ejects the former tenant of the place.

Now we have only to ask what is the time and speed with which a mass will descend from this place to that; and we find that it is the speed due to the height through which the centre of mass has to pass in descending from its first to its lowest place, or a speed of 16 feet a second, a fall of 4 feet, in half a second of time. In other words, the *speed of the transfer of force* is a speed of *ten miles* an hour, being the speed due to half the highest depth.

We see, therefore, that the descent of a raised mass of water can transfer its force forwards, by displacing an equivalent column at the known pace of the fall of the mass from centre to centre.

That is the speed of the effect propagated; we may next seek the manner of propagation. The propagation is the transfer of the force forward; the manner we must measure by the vertical forces and motions.

The vertical motions we already know are represented by the versed sines of the depth of fall and rise, which with equal arcs correspond to equal times. (See Diagram VI. Pl. II.)

The first stage of fall will therefore take place in the first instant of time, through the versed sine of the first arc of the time circle. (See Diagram VII. Pl. II.)

The second stage of fall will take place in the second instant through the versed sine of the second arc of the time circle.

And so on all through the half round of the time circle till the last fall to the level.

We have thus given the exact change in length and depth, which each water column undergoes, during its transformation from high to low, and during its transformation from low to high, and the place of each atom at each instant as shown on the time circle.

We have next to consider that these water columns form a series in two (or four) groups. (See Figs. 6 and 7, Pl. III.) :—

The rising group—six.

The falling group—six.

We may now examine the rising group :—

The front level : column one rising.
Column two risen 2nd stage.
Column three 3rd stage. Column four 4th stage.
Column five 5th stage. Column six 6th stage.

This transmission of force and changing form, with the full speed due to height of centre of falling mass, above centre of fallen mass, is in agreement with the known phenomenon, that in whatever line or path moving force is directed, and however slow or fast it moves during acceleration, the ultimate velocity of transmission will be that due to the length travelled along the line of force, quite regardless of the speed along the line of direction.

Hence it comes, that whether the water be deep and the wave shallow, or the water shallow and the wave deep, the speed of transmission will be the same so long as the sum of the wave and the water depth remains unchanged.

But there exists a second nature of horizontal motion quite different from this transmission of force and form, although simultaneous with it; it is

The Horizontal Translation of Water Mass.

In the former case there was a change of form and place, which consisted of given particles changing shape and place, by centres falling and rising with given speed.

In the new case we find one group of atoms, by the force of their fall, moving a larger group of atoms, and removing them to a distance, and leaving them there at rest. These form the translated mass.

We must here distinguish the moving mass from the mass removed. The mover is the part of the mass above water; the moved mass is the whole, under and above. The moving power is merely the fall through the wave height, the moved mass extends down to the full depth. The moving force is the same as the force of transmission, only the transmission was merely force in velocity, going from one mass to an equal one. Here the force is laden with a heavy burden; the weight of this burden slows the speed of translation, and diminishes the distance travelled. If a moving power of one is laden with an inert burden of four, it can only do quarter the distance in given time, and in every case the distance done in given time will be inversely as the load.

It will thus follow, that if a wave of translation 3 feet high be transmitted through still water 5 feet deep it will change place and travel forward 16 feet in a second; but the total mass of 8 feet will only attain three-eighths of that rate, or 6 feet per second; and as the motion will not be uniform, but accelerated and retarded as the versed sines of the time circle, the space described will be one diameter of acceleration and one diameter of retardation, or in a given ratio to twice the height of fall.

The translation of mass effected on the water is measured by the time circle of fall, the transmission of force and form by the circle of depth of water.

The time of the complete transport of this mass will be measured by the circumference of a circle, greater than the diameter of the circle of fall in the ratio of the height of fall to depth of water, or the circuit of depth as diameter.

This interval gives at the speed of transmission the wave length.

Diagrams and Examples of Waves.

Plate II.

Diagram I.—Arcs of the circle represent flow and measure of time—say tenths of a second.

Total circumference, 3·2 seconds or 32-tenths.

Divisions of the tangent lines represent rectangular co-ordinates of each arc; or vertical and horizontal elements of uniform motion in equal arcs of the circle; or versed sines.

The figures are used as convenient approximate types of the versed sines. The co-versed sines, tangent, are when infinitesimal, equal to the arcs.

Each chord of an arc has the same direction as each radius in the succeeding quadrant.

The versed sine 2d differences are greatest near zero; become uniform half-way. The differences become uniform at the radius and then diminish to half-way. The 2d differences again become greatest at the end of one vertical line.

The versed sines grow as the square of the chords of the arcs, from zero, become uniform and equal to the arcs at the tangent, and diminish as the squares at the end.

These versed sines give, as the diameter of the ⊙, 100; and the circumference of the ⊙, 320.

Diagram II. gives a circle of given vertical radius—say 50 inches—through which a body falling freely from rest will pass in half a second of time, and acquire a speed of 200 inches per second, or 16 feet 8 inches.

The arcs representing equal times. The radiant chords represent speed and motion by gravity along each chord. Each chord of the semicircle is parallel to the chord of an arc. Each chord measures relative speed as well as absolute force and distance. Space described in equal times is measured by versed sines of equal arcs, which are as the squares of the chords.

Velocities acquired at the end of each fall are those due to the height fallen, without reference to direction. The heights fallen are the versed sines. The velocities due to height are, therefore, as square roots of vertical heights, or *as the chords*.

In other words, the velocities at the end of the times are those due to the vertical fall (the fastest), without reference to the slowness of the previous fall.

Therefore, the vertical speeds on arcs, chords, and diameters are, at the same height, all alike, and are as the square roots of the versed sines.

Diagram III.—Times represented by arcs of a circle whose vertical diameter represents height of fall, are represented also by arcs of a semicircle of double diameter. The arcs of the circle and of the semicircle are of equal length.

The chords of these circular arcs and the sines of these semicircular arcs are equal.

The vertical semicircular sines may be taken as lines of vertical descent of water columns, &c., descending freely by gravity.

The moving force in each vertical water column is as its height.

The distance to be descended in each vertical column is as its height. Each distance being in the ratio of each force, all will be done in one time.

If, therefore, a series of masses were to descend along the chords of the circle, and a series of water columns were to descend vertically along the vertical sines of the semicircle, all would reach the level surface at the same time.

The water columns are, therefore, isochronous with the chords as lines of motion.

Diagram IV.—At each degree of descent there is perfect synchronism between the chord particles and the column particles. When each is half-way down its chord, each other is half-way down its column.

While the former keep rank and time in circles of diminishing radius, the latter leave their semicircle and take synchronous places in semi-ellipses whose vertical ordinates are equal to the synchronous chords.

Thus, each particle in the vertical column keeps time with its summit particle, and in each instant all move in harmony and in like proportional speed. Thus, the water columns keep the times of the circular arcs, and move down through the lengths of the circular chords, and keep the speeds due to the paths in the chords, though the directions are quite different (see later on).

It is also to be noticed that the total time occupied in motion through each vertical path and each chord, is the time of total free descent through the vertical diameter of the circle by gravity. Say 1 foot, $\frac{1}{4}$ second; 4 feet, $\frac{1}{2}$ second; 9 feet, $\frac{3}{4}$ second, 16 feet, 1 second.

Diagram V.—A couple of water columns, connected and full to the water-level, are vertical, equal, open above and united below. Then, by force, one column is raised, kept at rest, then let free. It descends in equal times through versed sines, accelerating to half-way down.

By force of this descending column the other column ascends above the water-level in equal times through versed sines, accelerating to half-way up.

The two meet half-way on a new level. Neither acts on the other, both are free; but both are in motion, full swing—one up, the other down. This swing force carries one upwards as far again as it rose, one downwards as far again as it fell. Both go on at diminishing speed in same times as before, but in reverse order. The two columns now rest in exchanged state.

Diagram VI.—Shows the successive stages of rise and fall of many water columns, in the same manner as the Diagram V. shows the stages of one couple of columns.

There are eight descending columns on the left, and their corresponding eight ascending columns on the right. The measures are taken vertically from the circle of isochronous rise and fall, and are transferred to their due places in the semicircle of translation shown apart in Diagram VII.

Diagram VII.—The versed sines of the circle of equal times, showing the vertical ascent and descent in successive instants, are transferred to the semicircle of isochronous water columns (III. and IV.). Here each vertical summit shows the place of each column all at the same time, and also the height of any one column at successive equal times. This diagram unites the times of vertical ascent and descent with the places of isochronous water columns in a semicircle of synchronism.

Diagram VIII.—Shows the hydrodynamic conversion of the vertical forces and motions of water columns into horizontal forces and motions.

The wave taken as example is in water 6 feet deep and 2 feet high, when D + H = 8 feet. A vertical column, confined, is 8 feet high; when freed forward it descends accelerating to 7 feet, retarding to 6 feet where it stays. The force goes forward 16 feet in a second, and is stopped there, then raises a second vertical column, which is then enclosed, like the first, having risen successively to 7 feet and to 8 feet by stored force, or energy. Left free, it again descends and goes forward to a third stage, and so on, ever forwards. The moving force is shown in the absolute conversion of a weight and volume of water one foot above the level into an equal weight and volume of water three feet below the level. This energy is transferred by hydrodynamic descent and horizontal displacement, and the velocity due to height is 16 feet per second.

Plate IV.

Diagram IX. shows the wave form resulting from the simultaneous propagation of the force in a straight line along the smooth surface of still water, and the synchronous rise and fall of the water column, in the versed sines of the time circle. The time circle also enables us to determine the exact wave length. Diagram VIII. has given us the speed of wave travel from instant of starting to instant of stopping. We still want to measure the time between starting and stopping; this is the time required for descent and ascent. Now descent takes place in two equal stages, or quadrants of the time circle; ascent takes place in two quadrants also.

If the ascents and descents were falls by free gravity, their times would be merely the times of free fall, retarded fall, free rise, and retarded rise. The time would be four times that of one height fall. But the time flows uniformly as the arcs of the circle of which the rise and fall are as the versed sines. While, therefore, the versed sines correspond to one second of time, the uniform time arc is 1·57, and for two seconds of time the time arc is 3·1416 seconds. Thus we have represented by the versed sines the heights, vertical rise and fall, and the times of rise and fall by the corresponding quadrants.

The true horizontal wave speed due to height and depth is thus given in Diagram IX. The true heights in successive times are given by the time circle of half wave height as radius. The wave period is given by the synchronous arcs measured on the circumference of the time circle—being 1·5708 for the half, and 3·1416 for the whole wave period.

The period × the speed give the wave length in feet.

Diagram X. shows, in connection with Diagram IX., the wave volume in two places,—on the face and in the deep.

On the water face.—The depths are as shown in Diagram I.: 1, 4, 9, 16, 23, 31, 40, 50, 60, 69, 77, 84, 91, 96, 99, 100. The volume is 100 × 50 = 5,000, volume unity. This is the rising wave. The whole wave volume is 10,000 W.L.

In the deep.—The successive displacements are as the above series, only horizontal and diminished in the ratio of W.L. to depth. Each water column successively acquires the horizontal speed due to the increasing height, and, the times being equal, the speeds as the $\sqrt{}$. of the heights, and the spaces are as the squares of the speeds, the distances travelled are as the versed sines of the semicircle of translation with the circle of time.

Volumes *f g h i k*...in one and in other are equal. Spaces described are as heights due to speed in both cases. The volumes or areas are as—

$$\underbrace{1 + 9 + 23 + 40}_{73} \text{ and } \underbrace{60 + 77 + 91 + 99}_{327}$$

$$(3) \underbrace{\qquad\qquad}_{400} (13)$$

Diagram XI. shows the genesis of the wave by a succession of forces creating the succession of heights due to the required velocities.

Diagram XII. shows the work done by the ship's bow in removing the water. See Plate IV., Figs. 14 and 15.

The horizontal section shows the wave water-lines of the ship, and also the extent of horizontal motion given to the water moved outwards in each successive half-second of time.

The vertical section shows the heights of the water as rising and falling above the water-level in successive instants; these heights being the heights due to the velocity given to each water column as displaced by the bow. This bow wave is a permanent phenomenon, remaining always unchanged in size and place on the ship, so long as the speed is constant and the depth of water unchanging. Although this bow wave is a permanent phenomenon, the water particles of which it is made are all changed in each wave-length of the ship's travel.

The cross section shows the end view of the bow wave in contact with the ship's skin. It also shows the section of the wave which is sent off outwards from the ship, as the consequence of the ship's displacement: and which is the wave of translation, carrying off to a new place the water displaced during the transit of the ship's bow.

Examples and Calculations.

We are now able to calculate the elements of the solitary wave of translation.

One Example.

Given, the depth of still water,
Given, the height of wave summit above water level,

Depth	6 ft.
Height	2 ft.

I.

Find the Speed of Transmission of the Wave along the surface of the water.
Sum of height and depth—

$6 + 2 = 8$ ft. "Summit depth."
Half of Summit depth, 4 ft.

The velocity due to the free fall by gravity through half the summit depth of four feet is—

16 feet per second; or,
10 miles an hour,* nearly.

That is the speed of transmission of the wave ... ... (1)

II.

Find the Time of ascent and descent of the water columns.

Wave height	2 ft.
Half height	1 ft.

Two feet fall { Time of fall, $\frac{1}{4}''$ Space, 1 foot (accelerating).
Time of fall, $\frac{1}{4}''$ Space, 1 foot (retarding).

Two feet rise { Time of rise, $\frac{1}{4}''$ Space, 1 foot (accelerating).
Time of rise, $\frac{1}{4}''$ Space, 1 foot (retarding).

* I take a fall of 16 feet in a second, or a speed of 32 feet in a second, as convenient numbers. Each place on the earth has its own number depending on latitude or height, and a few eighths of an inch have to be added for each place. Ex.: Greenwich, 16·09.

These are the times of free rise and fall by alternate accelerating or retarding gravitation.

But in hydrodynamic Transmission each column is the mover of a second like itself, or is "handicapped" with a second weight equal to its own. (See Diagram V.)

Times of ascent and descent when carrying double load.

$$\left.\begin{array}{l}\text{Two feet fall}\left\{\begin{array}{l}\text{Time of fall } \tfrac{1}{2}'' \text{ (accelerating)}\\ \text{Time of fall } \tfrac{1}{2}'' \text{ (retarding)}\end{array}\right.\\ \text{Two feet rise}\left\{\begin{array}{l}\text{Time of rise } \tfrac{1}{2}'' \text{ (accelerating)}\\ \text{Time of rise } \tfrac{1}{2}'' \text{ (retarding)}\end{array}\right.\end{array}\right\}\begin{array}{c}\text{Same force}\\ \text{moving}\\ \text{double mass}\end{array}$$

$$\left.\begin{array}{l}\text{Falling and rising free, the whole time made up, } 1''\\ \text{Falling and rising laden the whole time makes up, } 2''\end{array}\right\} \quad \ldots \quad (2).$$

But the ascents and descents in hydrodynamic synchronism by their descents and ascents not being made through spaces as the squares of the times, but through spaces measured by the versed sines of the time circle, whose arcs measure time by uniform speed—

Speed in the time circle is that due to the fall through the radius, and that speed describes uniformly a a space of 3·1416 feet, while the fall by gravity describes only two feet.

The time, therefore of a uniform motion going on with the speed time due to half the height of the wave is not 2″ but 3·1416″.

Or, the space described by uniform motion with speed due to half the wave height is greater than that described by wave descent in the ratio of 1 to 3·1416.

III.

Find the period of one complete cycle of solitary wave motion.

We have found the complete period of rise and fall of water columns, considered merely as masses falling freely by gravity; or, in balanced couples, each alternately acting as moving force and as dead load.

But the ascents and descents of water columns balanced are not made by the full force of gravity acting uniformly throughout the wave fall and the wave rise.

The falling force diminishes with the falling height in hydrodynamic synchronism. (See Diagrams V., VI., and VII.)

The full force of free gravitation would occupy $\frac{1}{4}'$ in descending one foot, but by this hydrodynamic diminution of force with distance from 0, the space passed over in given time is diminished and the time taken for the given space is increased.

Now the speeds created by uniform force are correctly represented by arcs of the time circle of which half the wave height is the radius.

Now the path described by uniform motion in the time of the fall through versed sines of the circle is truly represented by the corresponding arc of that circle. The sum of all these versed sines through one foot of fall is the radius of this circle, which in free fall occupies $\frac{1}{4}''$ and in laden fall $\frac{1}{2}''$, and the sum of times measured by the corresponding arcs of the synchronic circle are greater in the ratio of the radius to the quadrant, or as 1″ : 1·5708″. When laden by the doubled mass this ratio becomes doubled, or 1′ : 3·1416″.

In other words, The time of free fall through half the wave height, being quadrupled for double fall and double rise, is doubled again for laden hydrodynamic coupled columns, and increased to more than triple by their isochronism. Or in this case as 1″, 2″ and 3·1416″ ; 3·1416″ is therefore the duration of the complete cycle of solitary wave motion when the half height of the wave is 1 foot or the height due by gravity to $\frac{1}{4}$ free fall—

$$\text{Cycle} = 3{\cdot}1416'' \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad \ldots \quad (3).$$

This cycle of rise and fall in 3·1416″ does not change with varying depths of water.

IV.

Find the length of one complete solitary wave.

We have formerly seen that the speed of wave transmission is that due by gravity to the fall through half the summit depth, or to half the sum of still water depth and wave height.

We have taken this speed in this case of two feet wave and six feet deep as $\frac{6 + 2}{2}$ and have got as speed of transmission sixteen feet per second, or speed due to $\frac{3}{4}''$ free fall: see (1).

If we now reckon the length the wave cycle of motion will cover along the surface of the water in the duration of complete rise and fall in hydrodynamic synchronism; and with the speed of transmission given in (1), we get the length of the wave of 3·1416″ cycle in eight feet summit depth = 3·1416″ × 16 = 50·2656 feet (4), or 3·1416″ × speed due to free fall from summit depth=length of complete solitary wave, 50·2656 feet. (See Diagrams IX., X., and XI.)

V.

Beyond this cycle of phenomena on the water face, forming the wave, we have still to measure the work done in the water under the surface.

A large mass has been raised up, moved forward, lowered down, and left at rest in a new place. What is this mass? how was it moved? how far has it been removed?

This water-mass removed consists of the whole volume of the water forming that part of the wave which is raised above the still water level. This mass is sent up from beneath; its quantity may be measured by the wave length and half the wave height.

It is 50·2656 cubic feet.

It weighs $\frac{1}{3}$ over $1\frac{3}{4}$ tons.

The distance it is carried is—

$$\frac{50.2656}{6} = 8{\cdot}2832 \text{ feet forward.}$$

That is the work of water translation done by the wave, on each foot of thickness, or breadth of channel, or ridge of wave, or

$$\left.\begin{array}{r} 50{\cdot}2625 \text{ cubic feet water} \\ 1{\cdot}396 \text{ tons mass} \\ 8.2832 \text{ feet translation} \\ \text{in } 3{\cdot}1416 \text{ seconds of time.} \end{array}\right\} \quad \ldots \quad \ldots \quad \ldots \quad (5).$$

Thus as the wave glides forward along the surface of still water it removes each successive vertical column of water into a new place, over eight feet forward, keeping it upright and leaving it upright. While each column travels forward the pressure it endures increases its height in the first half time and restores it in the second half time to its original height at rest; this changing height being always the height of the part of the wave the column occupies, measured from the bottom of the water.

When the wave has passed over, all the water columns are left upright; all even under the level of the water face, and all at rest in their new place. (See Diagrams X. and XI.)

TABLE OF DEEP-SEA WAVES OR GROUND SWELL.

Calculated on 32 Feet as Standard.

Sea Depths.	Wave Lengths.	Wave Time.	Transmission Speed.
Feet.	Feet.		Feet.
32	201	6·28	32
50	314	7·85	40
72	452	9·42	48
98	614	10·99	56
128	804	12·56	64
162	1,016	14·15	72
200	1,256	15·70	80
288	1,808	18·84	96
338	2,122	20·42	104
450	2,826	23·45	120
512	3,216	25·12	128
648	4,076	28·31	144
722	4,536	29·82	152
800	5,026	31·41	160

DISCUSSION.

Mr. C. W. Merrifield, F.R.S., said: I am not disposed in any way to trespass on the time of the meeting, but I feel sure that I am expressing not only my own feelings, but those of many gentlemen present, in stating the great satisfaction with which I have listened to so experienced and able an authority as Mr. Scott Russell, going over again the old ground of his youth, and trying to make familiar to us the principle which led him to great discoveries—great especially at the time when they were made, when harmonic motion was very little understood, and when the synchronism of waves was scarcely understood at all, except in the crudest form, and when there was no connection established between the motion of waves and the displacement of ships in motion. There is always very great satisfaction in hearing the words of first inventors on those subjects. In my experience as a learner, I have always derived much more information from the first editions of books, however imperfect, than the more finished editions which afterwards were carefully worked down by the authors or their successors; but I must say, whether or not we are able to assent in every detail to Mr. Scott Russell's illustrations—because I fear, in some respects, according to my notions, they must be called rather illustrations than demonstrations—still they are of a character which is best expressed by the French word *fécond:* they are pregnant illustrations. There are, however, one or two points of difficulty, and it always struck me that there was a little difficulty about those points, in the exact understanding of Mr. Scott Russell's hypothesis, and one of these is very clearly developed, and I am very much obliged to him for so clearly developing it, in the paragraph marked 1, page 2 of his paper. You will see how very carefully he defines that there is no room for the water to escape below the vessel, and consequently that the whole escape is necessarily at the surface. As to that, it has always appeared to me that he proved rather too much; that if you confine a solid body moving in an absolutely closed receptacle filled with liquid—take for instance a closed glass cylinder, with an iron body in it, compelled to move

by a magnet—according to this demonstration it would be impossible that it should move at all, I think the truth is between the two, and the theory is to be a little qualified by the question of the lines of gliding gone into so carefully by Professor Rankine. It is well known that among those lines of gliding, a particular case is Mr. Scott Russell's curve of sines, and another particular case, under certain altered circumstances, is the trochoid, or wave of the second degree. It is not, and has never been, clear to me that Mr. Scott Russell's elucidation of his theory was an absolute demonstration of the observation, and, moreover, when we come to the wave of translation, as far as modern mathematics go, I think it is not at all clearly established that it is an absolute form of equilibrium. I am speaking now from memory; I have not referred to books on the subject recently, but I think it is clearly demonstrated by Green that the wave undergoes secular degradation, unless the motion is excessively small. When that is so we have a good many coincidences. In that case the trochoidal system of waves, indicated by Mr. Scott Russell and Professor Rankine, not only becomes identical with the waves of a perfect fluid, which have been defined on a totally different hypothesis, as indicated by Professor Stokes, but also, in that case, with certain other modifications of the trochoid, they become identical with the sinuschoid, and we get the case of small motions when the simple superposition identifies a great many things, and simplifies the whole problem in a way that does not occur in water. At the same time we must all feel obliged to Mr. Scott Russell, however we may disagree with him in detail, for having given us this extremely valuable elucidation of his theory, and I think, when a history of this subject comes to be written, perhaps by some of our descendants, we, and especially the younger ones among us, will be proud to be able to say, We have heard Mr. Scott Russell explain his own theory.

Mr. Dixon Kemp: Mr. Scott Russell has confessedly written his most interesting paper with the object of disproving a recently-advanced theory, that an important element of "least resistance" is, that the forebody of a ship should be of the wave form. To him, however, the all-important condition is, that the boundaries of horizontal planes in the forebody should be of the wave form or character. He therefrom, as I understand him, assumes that a ship which has these lines in her forebody delivers successive blows on the water, which tend to produce on the water's surface a wave of a particular form or natural form. The ship as she advances pushes away progressively layers of water, and as there is no room for the reception of these layers in the surrounding element abreast and below, they come to the surface in a wave, spread over a large area, and corresponding in form to the order in which they were pushed away. If, however, the bow has not wave lines, but full lines and full vertical sections, the blows from the bow instead of being delivered in a horizontal direction, are given in a more or less downward direction, and the water is thus forced upon an imaginary plane, which is immovable because it is "full of water below." The water that is struck by the bow, finding that it cannot get below, turns as it were on the imaginary plane, or is deflected in an upward direction, and ultimately finds its way to the surface in an ill-shapen mass, not all of the true wave form, such as the wave of translation ought to be. That water displaced by a ship is forced to the surface we all know, and so far the exposition seems a perfectly rational one, but for many reasons I think that the stream line theory of the late Professor Rankine, as propounded by Mr. Froude, is more in accordance with the true character of the disturbances which take place in the surrounding fluid during the passage of a ship through water. The ultimate conclusion of this theory is, that the magnitude of the wave-making resistance is dependent rather upon the length of the bow, than upon any particular form of horizontal lines or vertical cross sections which the bow may have. Mr. Colin Archer, who last year in this hall profounded the wave form bow theory, adopted this view of the case, and contended that so long as the stream lines were diverted in quantities that accorded with the proportions of the wave form, it

mattered little what the horizontal lines in the bow of the vessel were like. Mr. Scott Russell, however, says that he has by experiments, made years ago, disproved all this; but, whilst accepting experiments as generally convincing I am in this case, from certain facts which are within my knowledge, inclined to doubt their trustworthiness. Now, yachts are vessels which are unencumbered with any parallel length of middle body, or actual "straight of breadth," as shipbuilders term it, and are therefore peculiarly fitted for testing the wave line theory, or the wave form theory. During the last ten years I have had the opportunity of examining many yachts, which were built specially for speed, and whilst endeavouring to trace causes which contributed to their speed qualities, I could not help being struck with the fact that the wave line was more or less present in the horizontal lines of all which were noted for their speed. Some of these yachts had these wave lines in a pure form; in others the lines only approached the wave form; but still the hollow line was always present in some degree. Occasionally I came across a yacht with perfect wave lines, but which was not noted for speed. However, as I found some satisfactory cause for her deficiency in this respect, my conviction was mistaken, that the speed qualities of a yacht depended, so far as the form of the forebody went, upon her bow having wave lines, or lines of the wave character. But during the last two or three years yachts have been built with perfectly straight lines in the bow, and in some cases with lines which were actually convex, and these yachts are distinguished for their speed, even compared with yachts which have pure wave lines. Thus we have yachts with every variety of lines in their bows—wave lines, modified wave lines, straight lines, and what may be termed parabolic lines, and the upshot of all these wondrously shaped lines is, that they only produce slight variations in speed. This certainly was calculated to shake one's faith in the value of wave lines, and last year Mr. Colin Archer roundly stated that wave lines had nothing to do with the speed qualities of a ship, but that it is necessary that the bow should be shaped progressively in the wave form, so that the ship as she advances shall push away a quantity of water at the exact stages necessary to build up a wave of the natural form. I was much struck with Mr. Archer's theory, and during this year have applied it to those yachts which I have before spoken of. To my satisfaction I found that all the yachts with wave lines, or modified wave lines, or with straight lines, or parabolic lines, which were noted for their speed, had bows of the wave form; or, in other words, the displacement of their forebodies was disposed longitudinally in the wave form; but those yachts which had wave lines, but which were not noted for their speed, had forebodies which were very great departures from the wave form. One of these yachts not noted for her speed, although possessing wave lines in a more or less modified extent, was the *Titania*, built by Mr. Scott Russell; but a curve which exhibits her longitudinal progression of displacement, is one very much fuller than is the wave line or curve of versed sines, so much so that there is nearly a third more displacement at the mid length of the bow than required by the wave form theory. The *America*, on the other hand, although having much straighter bow lines than the *Titania*, has a forebody of the perfect wave form. The conclusion I arrived at, then, was this—that the using of wave lines in the bow often produced a forebody of the wave form, and when that occurred good speed qualities resulted; but wave lines without the wave form of forebody were of less value than straight lines united with the wave form of forebody, and that the very best results are obtained when modifications of wave lines are combined with the wave form of forebody.

The President (who took the chair during the reading of the second portion of the paper, on Friday evening, April 4th): I almost hope there will be no more remarks made upon this paper, because, although I am sorry to interpose any bar to discussion, really time must be considered to some degree. I hope my friend Mr. Scott Russell will not be very much offended with me if I venture to make three remarks on the precedent he has now introduced. In the first place, gentlemen, I must say that it appears to me (I am speaking with great deference, and I am sure there we shall all agree)

that all the intelligent men who assemble here must feel pleasure in listening to the ability with which Mr. Scott Russell treats subjects of this sort; and he has never shown that ability more than in the paper we have just heard. However, I must say, to my mind it is a question of considerable doubt—I will not say more than that—whether the paper we have heard from Mr. Scott Russell comes within the subject of Naval Architecture at all; and I think it is a question worth our consideration, having regard to the limited time at our disposal every year, whether we should not discuss subjects of real practical importance with regard to the construction of our ships, and the rules of Naval Architecture. I am further led with very great regret to say, that in my humble opinion—and of course it is my duty in this chair to think only of what appears to me to be for the interest of this Institution—that I doubt whether it is for the interest of the Institution that rules once made should be entirely disregarded. If our rules are not to be observed, let us put a sponge over them, and let us not profess to have rules, because if those rules are to be held good for one man, they ought to be held good for another. Although I should be sorry if anything I should say should hurt Mr. Scott Russell's feelings, or those of anybody else, I do not think it is for the interest of such an Institution as this that any man, however able he may be, should assume the right of violating the rules under which he acts. It is with very great regret, especially having doubts whether such a subject is germain at all, that I find the time allowed by our rules for three papers consumed in the reading of one. I cannot forbear making these observations, although it is with great pain I do so. Those, gentlemen, are the remarks which I have felt myself called upon to make upon the unreasonable time which has been occupied to-night with this our first paper.

A FEW YEARS' EXPERIENCE OF THE SCREW PROPELLER: ITS LESSONS AND RESULTS.

By Arthur J. Maginnis.

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, N. Barnaby, Esq., C.B., Vice-President, in the Chair.]

Of all the numerous matters connected with steamships which have engaged the attention of scientific men, that of the Screw Propeller seems to be the most intricate and incomprehensible.

A study of the numerous papers and articles which have been written, reveals how widely opinions differ on its many subjects, and also points out the strange fact, that experience (the most perfect instructor in other steamship matters) has up to recently, taught but little of the proper proportions of pitch, surface, diameter, or shape, although it has given severe and costly lessons of the great strength required both in the various sections of the boss and blades and in the mode of fastening it on the propeller shaft.

A brief glance over a few years' experience and study of the various propellers, and their effect on the steering of large ocean steamers, may perhaps throw some light on this great engineering enigma, and afford some information of practical use.

The construction of the propeller has of late undergone some change from the old style of a solid casting, owing to the great increase in the size, which entails considerable difficulty in obtaining cast-iron ones of correct pitch and sound metal, a large cast-iron propeller of suitable strength weighing from 21 to 25 tons.

The mode of construction now most in favour is that of having a boss with the blades bolted to it by flanges, as shown on Fig. 13, Plate V. This mode for propellers of 16 feet diameter and upwards is superior to the solid cast-iron ones, although there is the great objection to it, that the first cost is from two to four times greater; but notwithstanding this drawback it is gradually superseding the old style.

When this method first came into use objections were raised to it, owing to some of the bosses having split or broken, as shown on Fig. 13, Plate V., and also on account of the bolts breaking, which caused the blades to be lost.

Owing to the failures of the earlier cast-iron bosses, other metals have been tried, such as gun-metal, brass, steel, and in one instance wrought iron, but with the exception of the latter the great expense will always prevent their general use, as a moderate-sized boss of

those materials would cost from £90 to £140 per ton, or say from £600 to £1,000 for many sizes at present in use of cast iron.

The difficulty of obtaining a sound forging of 4 or 5 feet cube has up to quite recently prevented wrought iron being used, but the one in use at present, which is of the largest size, has given excellent results, and the price is not so high as to be a serious objection, being about 25 or 30 per cent. more; however, cast-iron bosses have proved to be most satisfactory, and when designed with a due regard to the sectional strength will be found most suitable and reliable.

The ordinary square-edged flange of the blade shown on Fig. 13, Plate V., being considered most objectionable on account of its increasing the resistance in revolving through the water, Messrs. Vickers, of Sheffield, have introduced a very neat and suitable style of boss; this arrangement, as shown on Fig. 12, Plate V., consists in having an angular recess in the boss into which the flanges of the blades are neatly fitted, so that the fillets of the four blades togther with a small portion of the boss form a complete circle.

The bolts attaching the blades to the boss have been the cause of very great expense and trouble, but fortunately the difficulty has been got over by the use of larger sections and best quality of steel; the nuts are now made of either steel solid-ended, as shown on Fig. 12, Plate V., or of the best Lowmoor iron, open-ended and covered with Portland cement to preserve them from the action of the water.

In coming to the subject of the blade of the propeller, the first thing to strike one is the fact that notwithstanding the numerous patented forms which have been brought out, the plain flat or oval-faced (on horizontal section) blade, introduced by Mr. Griffiths some years ago, is still the most in favour. This form is almost the only one in use on the large steamers sailing from Liverpool, after many trials of patented forms, a fact which would lead one to believe that, taken altogether, it possesses most of the various qualities requisite for a serviceable propeller.

The blades of the propeller have been made of almost every possible shape, form, material, number, and pitch; one proposing that they should be bent forward, another that they should be bent backward, another that the point of the blade should be in advance of the root, another that the root should be in advance, another that the blade should be straight, another that the horizontal sections of the blade should be concave, another that they should be convex, another that they should be the shape formed by an actual screw thread, another that they should have a large diameter, another that they should have a small diameter, and so on, until the owner or engineer of a steamship knows not which to act upon or what to do; and the strangest thing of all is, that they are supported by tabulated statements and records showing the superior advantages of the so-called improvements.

Fig. 10, Plate V., shows various shapes which were successively tried and found to be unsuitable, as their forms necessitated a thick medial section to prevent them breaking at or about the parts marked, as was the case with many. Another serious defect was the great vibration caused by them. Several of the principal styles of patented propellers have also been tried, but when the propeller required renewal they were given up on

account of not giving such superior results as would compensate for their extra expense and difficulty in making.

Of all the numerous shapes of blade tried up to the present time that shown on Fig. 9, or slight modifications of it, has proved the most satisfactory, and is now almost the only shape in use on the Transatlantic steamers. The advantages of this form are, that it has proved to possess the required strength with a moderate sectional thickness, it is thoroughly reliable in all weathers, and has lessened the vibration considerably.

Concerning the question of the number of blades a propeller should have, numerous trials have been made. The results given by the two-bladed propeller are that they drive the vessel well, but with almost every form of blade they cause vibration, and there is the decided objection to them of being easily disabled. Three-bladed propellers have also been tried, and have been found to be altogether unsuitable for large fine-lined steamships, as they cause excessive vibration. Six blades have also been tested, but gave no improvement, so that under all conditions the four-bladed propeller has been found to answer the purpose best, and is consequently the most in favour.

We now come to the much-vexed questions of pitch and diameter; the relative merits of pitches coarse or fine, uniform or varying, or of diameters large or small, have long been subjects of serious consideration to marine engineers. The writer has during the past few years endeavoured to obtain some reliable statistics bearing on them, as propellers of both different pitches and diameters have been used on the same steamers directly under his notice. The tables on pages 91, 92, which have been compiled from the logs of three of the largest Transatlantic steamers, will show in a brief form some interesting information; the voyages made with the different propellers being placed together when they have been made under similar circumstances, that is, the same length of time out of dry dock, the same state of weather, displacement, &c., as near as possible, so as to show the fairest comparisons under the many circumstances which materially affect the screw propeller. In addition to these, the results obtained from H.M.S. *Iris*, which are the most remarkable experiments yet made, are also given, similarly arranged.

By these results it will be seen that propellers of small diameter have in each case proved the most economical and effective: that on the steamship *Alpha* having given a reduction of 3 per cent. slip, and 111 tons of coal on round voyage, and an increase of ·3 knots per hour in speed; that on the steamship *Beta* gave a reduction of 2·4 per cent. slip, and 63 tons of coal on round voyage, and an increase of half a knot per hour in speed; that on the steamship *Gamma* gave a reduction of 2 per cent. slip, and 152 tons of coal on round voyage, and an increase of half a knot per hour in speed.

In the case of H.M.S. *Iris* the particulars are taken from the reports published in the *Times*; by these it will be seen that the smaller propellers proved much superior, having driven the vessel at a speed of 18½ knots per hour, which the larger propellers were totally unable to do; the smaller propellers also gave a speed of 16½ knots (the maximum of the larger) with about two-thirds of the indicated horse-power. The consumption on the trials has unfortunately not been published, but if we assume it to be 2 lb. per indicated horse-

STEAMSHIP "ALPHA."

Large Propeller—Diameter 23 ft. 6 in. Pitch at Boss 26 ft., at Periph. 33 ft. Mean 30 ft. Surface 124 sq. ft.
Small Propeller—Diameter 21 ft. Pitch at Boss 29 ft., at Periph. 30 ft. Mean 29 ft. 6 in. Surface 153 sq. ft.

Outward Voyage.									Homeward Voyage.							
Voyage.	Average Revolutions. Per Minute.		Average Slip Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.		Average Revolutions. Per Minute.		Average Slip. Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.	
	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.
A	53	54·8	11·9	6·1	14·7	15	873	905	52·7	54·8	11·7	6·1	14·7	15	925	825
B	53·5	56·1	10·5	9·8	14·1	14·7	1,016	915	53·4	54·8	6·6	6.7	15	15	940	885
Mean	53·25	55·4	11·2	7·9	14·4	14·8	944	910	52·5	54·8	9·1	6·4	14·8	15	932	855
Home	52·5		9.1		14·8		932		Out	55·4		7·9		14·8		910
Round Voyage	52·8		10·1		14·6		1,876			55·1		7·1		14·9		1,765
Results.					Large Propeller				52·8		10·1		14·6		1,876	
					Small do.				55·1		7·1		14·9		1,765	
					In favour of Small						3		·3		111	

STEAMSHIP "BETA."

Propellers as above.

Outward Voyage.									Homeward Voyage.							
Voyage.	Average Revolutions. Per Minute.		Average Slip. Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.		Average Revolutions. Per Minute.		Average Slip. Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.	
	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.
A	52·4	56·1	9·1	8·8	14·8	15·6	874	810	52·3	55·4	8·9	6·3	15·4	15·4	928	845
B	51·8	54·6	14·5	12·3	13·8	14·7	955	833	51	54·9	12	11·8	14·4	14.6	759	902
Mean	52·1	55·4	11·8	10·5	14·3	15·1	915	822	51·6	54·6	10·4	9	14·9	15	844	874
Home	51·6		10·4		14·9		844		Out	55·4		10·5		15		822
Round Voyage	51·8		11·1		14·6		1,759			55		9·7		15		1,696
Results.					Large Propeller				51·8		11·1		14·6		1,759	
					Small ditto				55		9·7		15·1		1,696	
					In favour of Small						2·4		·5		63	

STEAMSHIP "GAMMA."

Large Propeller—Diameter 21 ft. 6 in. Pitch at Boss 29 ft. 6 in., at Periph. 32 ft. 6 in. Mean 31 ft. Surface 128 sq. ft.
Small Propeller—Diameter 20 ft. Pitch at Boss 27 ft. 3 in., at Periph. 28 ft. 7 in. Mean 28 ft. Surface 124 sq. ft.

Outward Voyage.									Homeward Voyage.							
Voyage.	Average Revolutions. Per Minute.		Average Slip. Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.		Average Revolutions. Per Minute.		Average Slip. Per Cent.		Average Speed. Knots pr hour		Total Consumption. Tons.	
	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.	Large.	Small.
A	45·9	52·6	12	9·6	12·3	13·1	777	627	46·8	51·6	3·5	2·8	13·8	13·8	672	593
B	45·4	52·9	17·4	13	11·4	12·1	872	755	46·2	53·4	3·6	2·7	13·6	14·3	672	723
Mean	45·6	52·7	14·7	11·3	11·8	12·5	824	691	46·5	52·5	3·5	2·7	13·7	14	672	653
Home	46·5		3·5		13·7		672		Out	52·7		11·3		12·5		691
Round Voyage	46		9·1		12·7		1,496			52·6		7		13·2		1,344
Results.					Large Propeller				46		9·1		12·7		1,496	
					Small ditto				52·6		7		13·2		1,344	
					In favour of Small						2·1		·5		152	

H.M.S. "IRIS."

Large Propellers—Diameter 18 ft. 6 in. Mean Pitch 18 ft. 6 in.
Small Propellers—Diameter 16 ft. 3 in. Mean Pitch 20 ft. 6 in.

Speed per Hour.	Average Revolutions.		Indicated Horse Power.		Consumption at 2 lbs. per I.H.P.	
Knots.	Large.	Small.	Large.	Small.	Large. Cwt.	Small. Cwt.
12	65	61	2,561	1,837	45·7	32·8
16½	87·9	86	7,478	5,132	133·5	91·6
18½		97		7,734		138

	Speed.	I. H. P.	Consumption.
In favour of Small Propellers.	12 knots	724 less	12·9 cwt. per hour = 16 tons per day.
	16½ ,,	2,346 ,,	42 ,, ,, = 50 ,, ,,
	18½ ,,		

power, the saving of fuel effected at this speed would be about 42 cwt. per hour, or 50 tons per day, and at a speed of 12 knots about 13 cwt. per hour, or 16 tons per day.

From the foregoing it is evident that the small diameters give the best results, and also (excepting in the case of the *Iris*) that the existing pitches are a little too coarse, for if the speed of piston will allow it, a slightly reduced and more uniform pitch will be found advantageous in many ways, as was found to be the case with the *Gamma*, the piston speed having been raised from 460 to 520 feet per minute by the smaller propeller, and since the increase the engineers have reported the general working of the machinery to be greatly improved.

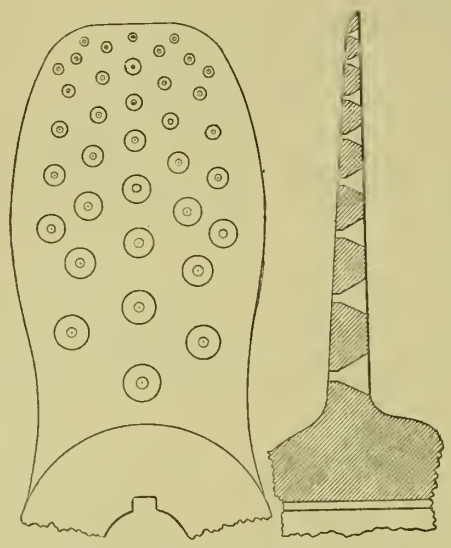
The superior qualities of the present favourite shape of blade has also served to bring about some of the foregoing improvements, especially to reduce the vibration; it will also be noticed that the form of blades which gave such good results on the *Iris* are almost similar to those now in general use on the Transatlantic steamers. The different propellers used on these occasions are shown on Fig. 9, Plate V.

The next subject to be noticed concerning the screw propeller, owing to the great expense which it causes, is the extensive corrosion which takes place on the back of the blades; and to so serious an extent does this corrosion take place that it renders the life of a large cast-iron propeller extremely short, being only about two years and a half when in constant service. Steel blades are also affected by it, but improved steel has lately been introduced, which has been found to last from three to four years.

The cause of this corrosion has, like every other matter connected with the propeller, given rise to much difference of opinion, the most general being attributed to the vacuum at the back of the blade; but as corrosion also takes place upon the driving face of the blade, along by the leading edge, some other means must be at work to have such an effect. Having carefully examined many propellers which had been condemned, owing to their corroded condition, it is the writer's opinion that the great cause of it is the formation of some acid by the mixing of the atmosphere and salt water, which attacks the iron; this mixing is caused by the descending blades of the propeller when crossing the top centre, having displaced the water, and also formed a partial vacuum at the back of the blades, which then draws in water highly charged with air, otherwise foam, and if the vessel be light, pure air, which will be carried down into the water by the blades.

Many arrangements have been designed to prevent this corrosion, such as fastening zinc plates to the backs of the blades, and also iron plates. Covering the blades with a coating of vulcanite has been tried, but it would not remain on, owing to the friction. A Portland cement coating has also been used, but with no better success. Another design which has recently been brought out in New York to prevent the corrosion, is to perforate the blades of the propeller with holes (as shown on sketch), the diameters of which decrease as they approach the circumference; the holes are also countersunk on the driving face of the blade. Numerous other advantages are claimed for this improvement, which

if on trial prove effectual will more or less revolutionise screw propellers. The usual course of the corrosion is shown on Fig. 10, Plate V., the blade A showing the driving side and B the back side, the darker parts being those where the corrosion is quickest.



The mode of attaching the propeller to the shaft is a point of which it cannot be said that experience has taught nothing, as almost daily accidents occur which show the necessity of great care being used when fitting on a propeller. If the boss be not a perfect fit on the shaft, it is not to be expected that the keys will be able for their work, nor is the propeller nut to be depended upon to keep tight.

Several cases have come under the writer's notice during the last few years, of disablement through apparent defect in the keys, whereas if the boss had been carefully fitted the keys would have been found amply strong. One of these cases was that of a new cast-iron propeller, which had been put on with the old keys. On the return of the vessel the propeller was reported loose on the shaft, and on examination it was found that one key was sheared across, and that the boss had been working to a serious extent: after this a careful fit was made and keys of the same dimensions fitted, with a result which proved they were of ample section. The method often adopted of putting the keys at right angles is open to serious objections, owing to the great difficulty they present of making the boss a proper fit, and should never be adopted with large propellers, but where there are two keys, which is always desirable, they should be placed opposite to one another. Another danger in the fastening of propellers is that of allowing too short a length of key, which renders it liable to be sheared across or to fracture the boss; the latter defect is often found to take place when the propeller boss is cored out in the inside, and the key only fitted in the forward end, as is generally the case.

The nut securing the boss on the shaft has in a few instances given trouble by slacking back and injuring the rudder post. To avoid this the nuts should in all cases be fitted on a screw of an opposite hand to the propeller; that is, if the propeller be right hand, the nut should be fitted left hand, and *vice versâ*.

At the present time much difference of opinion prevails as to the question of solid cast-iron or movable-bladed propellers; on this question the following incidents which have occurred will perhaps be of some service. Some time ago, a large Atlantic steamer, when shifting docks a few days before the date of sailing, fouled the dock wall with the propeller, and broke off one of the blades. This accident, though slight in itself, was enough to prevent her sailing, as to have a new propeller she would require to go into dry dock, whereas had it been a movable blade it could have been replaced when the vessel was afloat, and thus have

allowed her to sail at the appointed time. Another case was that of a large steamer on the South Pacific coast, which broke her propeller, and was detained some weeks until a caisson to fit the stern was built so as to get at the propeller, as there was no dry dock large enough to admit her. On the other hand, another steamer, with a movable-bladed propeller, broke all the blades when within a few days' sail of a Brazilian port, into which she sailed and replaced the blades by lightening the ship aft, the delay being one week, of which only two days were occupied in taking off the old stumps and putting on the spare blades which she had on board, the other days being occupied in discharging and replacing cargo. Other objections to solid propellers are their great size, which occupies so much cargo space when carried as spare, and in case of breaking off one blade the entire propeller has to be condemned; and again, an entire propeller has always to be kept in reserve, as it occupies so long a time making, say from eight to nine weeks; so that although the first cost of the movable style is from two to three times as expensive as the solid, the great advantages they possess of quickly and easily effected repairs (which prevent serious delays) have proved them to be the most useful. The table showing the particulars of the propellers of some of the largest steamers sailing from Liverpool will show the general practice and dimensions now used.

PARTICULARS OF PROPELLERS OF LIVERPOOL STEAMERS.

Name of Steamer.	Diameter.	Pitch.			Surface.	Number of Blades.	Material.		Weight.		Right or Left Hand.	Diam. of Cylinders.				Stroke.	Revols. per Min.	Speed per Hr. Knots.	Shape.
		At Boss.	At Perip.	Mean.			Boss.	Blades.	Tns.	Cwt.		H.P. No.	H.P. Dia.	L.P. No.	L.P. Dia.				
	ft. in.	ft. in.	ft. in.	ft. in.	sq. ft.											ft. in.			
Iberia	21 0	25 0	29 0	26 4	111	4	Steel	Steel	18	1	Right	1	56	2	78	5 0	52	14	Ordinary Griffiths.
City of Berlin	21 0	31 0	31 6	31 3	130	4	Cast	Iron	20	...	,,	1	72	1	120	5 6	55	15½	,,
Britannic	23 6	26 0	33 0	30 0	124	4	{Wrt Iron}	Steel	21	13	,,	2	48	2	83	5 0	52	15½	,,
Bothnia	20 10	26 6	29 7	28 1	113	4	{Cast Iron}	Steel	15	10	,,	1	60	1	104	4 6	52	13½	,,
Ohio	17 0	...	...	24 0	...	4	...	...	...	...	,,	1	57	1	90	4 0	60	13	,,
Sardinian ...	20 0	24 0	26 6	25 6	107	4	{Cast Iron}	Steel	16	...	Left	1	60	1	104	4 0	54	13½	,,
Gallia	21 0	26 0	33 0	30 0	116	4	{Cast Iron}	Steel	21	7	Right	1	61	2	84	5 0	56	14½	,,
Spain	20 6	Uniform.		26 0	120	4	{Cast Iron	Cast Iron}	21	4	Left	1	60	1	106	4 6	51	12½	,,

Of the many and varied ways in which a propeller may become disabled, it may be of interest to mention a few of the most remarkable. In 1873 the *Celtic* fouled some floating wreckage off the south coast of Ireland, and lost the entire propeller. In 1874 the *Scythia* struck a large whale, and broke off three blades. In 1877 the *City of Berlin* became disabled by the keys in the propeller boss having fractured the boss to their own depth, or in other words turned out the metal, as shown on Fig. 11, Plate V. Another peculiar disablement of a propeller is shown on the same drawing. This occurred to the *City of Chester* in

1878; as may be seen, the propeller broke through the boss, losing two of the blades, but the fracture not being large enough at the after end to clear the shaft's diameter, it hung in its place until the vessel arrived in port.

Throughout the year 1878 there were upwards of thirty-three cases of disabled propellers reported in one of the Liverpool daily papers (not a shipping), and no doubt this would be but a small fraction of the large number which had actually taken place.

Attention has been called in another portion of this paper to the square-edged flanges of the blades, owing to the great power absorbed in driving them through the water; another serious addition to them is the thick medial sections of the blades, which have of late years been made extra thick owing to the many breakages. It is to these points attention must be turned in the future, in order to increase the efficiency of the screw propeller by an improved mode of attaching the blades, which will allow of reduced size and different shape of boss, and the adoption of a reasonably priced metal of great strength combined with lightness, which will allow of a great reduction in the sectional thickness. A step in the right direction has been taken by Mr. Howden, of Glasgow, who some time ago, in a paper read before the Institution of Naval Architects, brought forward in a most lucid manner the great loss occasioned by the present form of propellers owing to the foregoing defects, and has designed a new principle of construction which bears about it a look of being up to its work, although men long experienced in the subject have stated that it looks weak about the root of the blade; still, when the great reduction in the useless area and the thin sections of the blade be considered, it may prove amply strong, a fact which a short trial on a large steamer would soon show.

As almost everything in the matter of speed, safety, and prestige which large steamship companies and owners value so highly rest upon the propeller being thoroughly reliable and efficient, it may be of interest to note what experience has taught as to the design of large propellers with the metals and forms in use up to the present time, rejecting the various ones proposed; for so much depends upon it, owners are exceedingly averse to trying experiments; as one of them remarks, "they prefer the safe to the scientific."

For propellers of 16 feet diameter and upwards, the system of having the boss and four blades separate is the best, the former being made of superior cast iron annealed, care being taken not to have it cored out too much, as this has caused the failure of many. The blades should be of the improved steel recently introduced, of the shape shown on Fig. 9, with a good substantial section, the thickness at the root being $\frac{1}{2}$ inch to $\frac{9}{16}$ inch for each foot of diameter: about 2 inches in from the tip the thickness should be about 1 inch or $1\frac{1}{4}$ inches. The points of the blades should be bent from 2 to 3 inches aft of the straight or perpendicular line of medial section, commencing about 2 feet 6 inches from the tip: the horizontal sections on the back of blade being convex, of increasing radii as they approach the tip, and on the driving or after face slightly convex at the root, gradually decreasing till they become straight. The pitch should not vary more than two feet at the most, and should be fine enough to allow the engines to work at a piston speed of from 500 to 560 feet per minute. However, these statements may be subject to many conditions, and

from the strange results which have of late been obtained from propellers, and which have overthrown the long-recognised rules and theories, it seems to the writer that on this subject no stated rules can apply, as almost every steamer requires a certain propeller, which unfortunately only well-watched trials will show or reveal, but it may be safely said that the engines as a rule are at present overweighted or overscrewed, the pitches being slightly on the coarse side and the diameters unnecessarily large.

We now come to the question relating to the propeller which has of late been most before the public, that is, the effect on the steering of the steamer. From a short experience and study of this important subject, the writer is of opinion that the effect is contrary when the steamer is starting or moving slowly ahead to that which has been generally stated and accepted. It has often been stated that steamers with right-handed propellers would (if the helm were kept amidships and a flat calm prevailed) go round in a circle to the right hand (or starboard), and left-handed propellers in a circle to the left hand (or port). The diagrams with the following explanations, will serve to render more clearly the opinions for the contrary being the case, when the steamer is just starting or moving slowly ahead, *i.e.*, that a right-handed propeller will make the ship describe the circle to the left hand and *vice versâ*. Plans of vessels with right and left handed propellers are shown with arrows pointing to the courses which each would take, and under two of these plans are vertical elevations of the propeller looking forward, the water above the centre line of shaft on each diagram being shaded light and that below dark for the sake of distinction; Figs. 1 and 2, Plate V., being the first case, when the steamer is just starting, and Figs. 3 and 4, Plate V., the second case, when the steamer is going ahead at a moderate or full speed. In the first case it may be seen that the water acted on by the top blade A has a most roundabout way of getting to the screw port, and is consequently much disturbed; so that the resistance to the blade is greatly decreased, as is also that to the ascending blade D, which, owing to the decreasing density, is gradually inclining to lift or push the water before it. Then turning to the other blades, it may be seen that the lower blade C is thrusting against the most dense and least disturbed water, and the descending blade B is gradually meeting with increasing resistance; the consequence being that as the resistance to the lower blades is much greater than that to the upper, the centre of shaft, being the centre of effort, will be inclined to move in the same direction as the upper blades, which have the lesser resistance (this direction is shown by the arrows); and as the stern of the ship contains or holds this centre of effort, it must of necessity tend to go in the same direction, which in the case of the right-handed propeller will be to the right hand, so that the vessel's head will go off to the left hand (or port). In the case of the left-handed propeller, for the same reasons, the stern of the vessel must of necessity tend to go to the left hand, and thus make the vessel's head go off to the right hand (or starboard).

In brief nautical language the result is, when steamers are just starting those with right-handed propellers will answer their starboard helm quickest, and those with left-handed propellers their port helm. Then, taking the second case, when the steamer is going at a moderate or full speed ahead, it will be seen that the water above the centre

line of shaft has, as Professor Reynolds expresses it, obtained an onward motion in the same direction as the vessel herself, and consequently, owing to this, the water presents to the upper blades of the propeller a much greater resistance than to the lower, so that in this case the directions of the arrows on the vertical elevations will be exactly reversed, and therefore the vessel's head will, if a right-handed propeller, go off to the right hand (or starboard), and if left-handed to the left hand (or port). In nautical language, steamers going at moderate or full speed ahead with right-handed propellers will answer their port helm best, and those with left-handed propellers their starboard helm best. On the other hand, or third case, when the engines and vessel are both going astern either slowly or at full speed, the effect of a right-handed propeller is to draw the steamer's head to the right (or starboard), and of a left-handed one to draw the steamer's head to the left (or port), that is, provided there is no wind blowing, and that the rudder is kept amidships, for if it be moved it will produce strange results, which I hope to point out later on.

The most important matter, however, concerning the action of the propeller on the steering is the effect brought about by the sudden reversing of the engines from full speed ahead to full speed astern, the vessel still having considerable headway on, as is the case when coming into collision, and it is under these circumstances that the peculiar effects have mostly taken attention; these may be to some extent accounted for by the fact that the water or current which acts upon the rudder is drawn by the reversing propeller in a direction directly opposite to that in which it would act upon it if the propeller were at rest, as is shown on the diagram Fig. 7, Plate V., which is a plan of a vessel supposed to have great headway on with her helm hard a-port, and the propeller (right-handed) working full speed astern. According to this it may be seen that the real action of the rudder under these circumstances is to turn the vessel's head to the left (or port), that is, in a direction directly opposite to that which had been intended; the plan alongside of it, Fig. 8, Plate V., showing the course or direction intended, and which would have been taken had the propeller been stopped and not reversed.

This peculiar action is also assisted by the different effects of the top and bottom blades referred to before, as the vessel's head will cant most and readiest when the rudder and propeller are acting in conjunction, or to speak more plainly, when the rudder is placed to cant the vessel's head in the same direction as the propeller would turn it.

These statements are to some extent borne out if the reports of actual trials made by Professor Osborne Reynolds on the steamer *Melrose*, in 1877, published in the *Engineering* papers, be correct. Figs. 5 and 6, Plate V., are plans showing these results. It may be seen that on the first experiment the steamer's head had turned twenty-eight degrees to the left (or port), the helm being hard a-port, and on the second experiment the steamer's head had turned forty degrees to the right (or starboard), the helm being hard a-starboard; the courses actually taken being in both cases directly opposite to that which the rudder under ordinary circumstances would have steered the vessel.

The additional effect to the steering given by the propeller's own action is shown very clearly in the second experiment by the greater angle (forty degrees) canted to, than in

the first, for in this case the rudder and propeller were acting in conjunction, that is, both tending to make the vessel's head cant to the right (or starboard). If the foregoing statements as to the effects of screw propellers on the steering of steamers be accepted, the following will be the various effects under all circumstances for steamers with right or left-handed propellers, so arranged as to be suitable to place in the wheel-house, for the guidance of the commanders and crew.

EFFECT OF SCREW-PROPELLERS ON THE STEERING OF STEAMERS.

RIGHT-HANDED.	LEFT-HANDED.
First case, Steamer and Propeller both starting Ahead.	
Propeller will drive steamer's head to port. Steamer will answer starboard helm quickest.	*Propeller will drive steamer's head to starboard. Steamer will answer port helm quickest.*
Second case, Steamer and Propeller both going Ahead Moderate or Full Speed.	
Propeller will drive steamer's head to starboard. Steamer will answer port helm quickest. Steamer will require a little starboard helm to keep straight course.	*Propeller will drive steamer's head to port. Steamer will answer starboard helm quickest. Steamer will require a little port helm to keep straight course.*
Third case, Steamer and Propeller both going Astern.	
Propeller will draw steamer's head to starboard. Steamer will answer starboard helm quickest.	*Propeller will draw steamer's head to port. Steamer will answer port helm quickest.*

That is, if helm be kept amidships and no wind prevails, as in a breeze the stern of a steamer will always seek the wind, no matter what may be the action of the rudder or propeller.

Fourth case, Steamer going Ahead, Propeller working Astern.	
If helm be kept amidships, propeller will turn steamer's head to starboard. If helm be put hard-a-port propeller will turn steamer's head to port. If helm be put hard-a-starboard propeller will turn steamer's head to starboard quickly and to a great angle.	*If helm be kept amidships propeller will turn steamer's head to port. If helm be put hard-a-port propeller will turn steamer's head to port quickly and to a great angle. If helm be put hard-a-starboard propeller will turn steamer's head to starboard.*

In this case the wind will not have much effect until the steamer has lost headway, and it must be noted that the position of the rudder is of paramount importance.

However, the first and fourth of these cases are, in ordinary service, the only ones which call for particular attention, more especially the fourth case, as the strange effects produced under these circumstances are almost always experienced when steamers are in danger of coming into collision.

It may not be out of place here to call attention to a fact which every steamship company and owner should take particular notice of, that is, the serious loss occasioned both to them and the engineering profession by permitting the available experience gained year after year on large merchant steamers to pass almost unnoticed and unrecorded.

In concluding this review of "A Few Years' Experience of the Screw Propeller," the writer begs to return thanks for the permission granted him to bring forward information which he hopes will assist in showing more clearly what has been "its Lessons and Results."

DISCUSSION.

Mr. William Denny: Mr. Chairman, it seems to me that the paper we have just heard is rather taken up with the question of the construction of the propeller than with the question of its efficiency, and that the competition for which this paper entered was essentially a competition on the efficiency of the propeller. In the paper itself, I can find no reference to the efficiency of the propeller excepting that shown in Figs. 9, Plate V., in which the general doctrine held is, that by decreasing the diameter, and following Mr. Froude's discovery—of decreasing the peripheral friction, you will get more efficiency. But Mr. Maginnis in treating this, although he has followed the great discovery that we now all know so well, has not taken care to qualify his statements upon this point, because it must be quite evident to you that if this decreasing process were to be carried on far enough, you would come to the boss, and then you would come to a very negative result indeed. Therefore it is quite useless to carry on investigations upon this point, unless those investigations are carried out accurately and minutely, and unless they are carried out definitely upon the measured mile—not upon voyages of ships, which I hold are most delusive things, because however nearly you may suppose the circumstances to accord, we all know very well that, even in fine-weather voyages, there are many things, as a captain of a ship can tell you, which make differences in the voyages of the ship, and we are apt rather to flatter ourselves with favourable results than to test ourselves with crucial experiment. The other point to which Mr. Maginnis refers is the eliminating of certain frictional elements in the working of the propeller by some small mechanical detail. Now in criticising Mr. Maginnis's paper, I do not rise to criticise the award of the Council, because I have not seen all the papers upon the subject; but I only think it is fair to draw the attention of this meeting to the fact that this paper does not, excepting in the most meagre way, attempt to meet the requirements of the competition.

Mr. John McFarlane Gray: Mr. Chairman and gentlemen, I am quite aware the subject of the screw propeller is one which to all studious engineers has had great attractions; and I, twenty-three years ago, went into the investigation of the screw propeller with reference to thrust and pitch, and found, as many of us have found, that we are bothered with the following water, and cannot deal with that quantitatively. I unburied some of these investigations in anticipation of this meeting, and read them over again, and I thought I would put them into shape. If for the screw propeller with its inclined blades there could be substituted a complete flat disc of the same circumference, C feet, to be pushed astern by a screwed shaft of pitch P feet, making N

revolutions per minute, and if the slip with this disc propulsion be S feet per revolution of the screwed shaft, the thrust would be

$$\frac{64}{32} \cdot \frac{NP}{60} \cdot \frac{NS}{60} \cdot \frac{C^2}{4\pi} = \frac{N^2 P S C^2}{22620}.$$

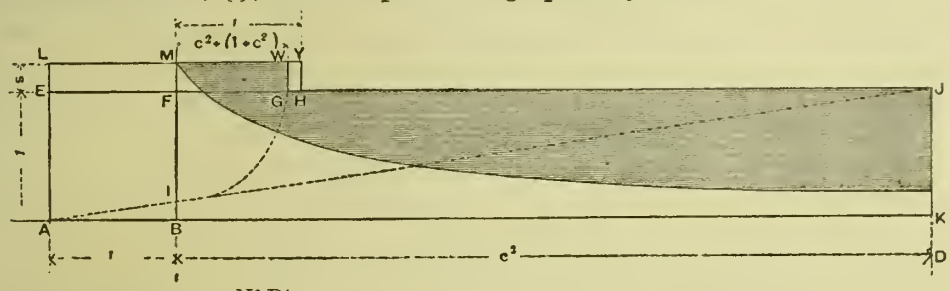
Or, writing

$$s = \frac{S}{P}, \qquad c = \frac{C}{P}, \qquad \text{disc thrust} = \frac{N^2 P^4}{22620} s c^2.$$

Applying the same reasoning to a screw propeller, and taking account of the obliquity of the blades, I get

$$\text{Screw thrust} = \frac{N^2 P^4}{22620} s \left\{ c^2 + s \frac{c^2}{1 + c^2} - (1 + s) \log_e (1 + c^2) \right\}.$$

The part within the brackets, { }, can be represented graphically, thus:



The shaded area multiplied by $\frac{N^2 P^4}{22620} s$ is the thrust in pounds. The diagram is for $c^2 = 6$, $s = \cdot 2$. Rankine's expression for thrust given in the Transactions for 1865 is a more complicated form of the same expression, and by substituting the same letters and re-arranging the terms the one can be transformed into the other. My investigation was quite independent of Rankine's, which was seen by me for the first time only this evening. The diagram enables us better to grasp the order in which variations in s, p, c affect the result. I have slightly modified the construction from that first exhibited by me, to clear the diagram of a negative area. If the fractional area W H be neglected, the diagram for a given slip may be extended to the right to represent the thrust factor for any diameter of screw with that slip. The thrust of a screw propeller as compared with the thrust of a plane disc of the same diameter is as the shaded area M W G J K to the area of the rectangle F J D B. The denominator in Rankine's formula comes out to be 22691 in the approximate rule he gives in the Appendix. When the letters used above are substituted that approximation becomes

$$\frac{N^2 P^4 s}{22691} c^2 \left(\frac{c - \cdot 8}{c} \right);$$

the part $\left(\frac{c - \cdot 8}{c} \right)$ being the mean breadth of the shaded portion of the diagram. The approximation is very close between $P = \cdot 9\,D$ and $P = 2 \cdot 1\,D$, the usual range nearly. The discrepancy between 22691 and 22620 is due to my using 64 and 32 merely instead of with the decimal place figures. Rankine's approximation is more conveniently applied when written thus. The thrust in pounds is nearly equal to

$$\left(4 \cdot 465 - \frac{1 \cdot 137\,P}{D} \right) K k (D^2 - B^2), \quad \text{or say} = \frac{36\,D - 9\,P}{8\,D} K k (D + B)(D - B),$$

in which K is propeller speed in knots per hour, k is slip in knots per hour, B is diameter of boss, D is diameter of screw propeller, and P is pitch, all in feet.

Mr. A. C. Kirk: I think it must be a matter of regret that Mr. Gray had not sent in a paper on this subject, but I hope he will supplement what he has said by writing out his remarks more fully.

Mr. J. Fortescue Flannery: Mr. Chairman and gentlemen, I should like to say a word or two, following the same course as Mr. Denny, about the application of this paper to the title which was set forth in the commencement of the competition. No doubt the Council in dealing with the question were obliged to decide on the merits of the papers, not because they exactly followed the lines that were set down by Mr. Carlyle, but because they had to take the paper which was the nearest practical approach to those lines. I am not aware what papers were sent in, nor have I any knowledge of their contents, but it seems to me very likely indeed that the Council were obliged to accept the paper most nearly like the one desired by Mr. Carlyle, or withhold the Medal altogether. No doubt that has been their view, and that has been the reason why Mr. Maginnis has had the Medal awarded to him. I must say I agree with what Mr. Denny has remarked, and knowing as I do the position which Mr. Maginnis holds, and the facilities he has for obtaining the very best knowledge on this subject, I cannot help saying publicly that I hope he, and those who were concerned with him, will see fit to allow this paper to be very much elaborated and extended, and to appear upon the Transactions in its more complete form or with an appendix. For example, we are told that these three vessels, of one of the first lines out of Liverpool, had certain results upon their voyages. The particulars of the propellers are given, but no particulars whatever are given in the paper of the machinery or form of hull. Now I think that the relationship of these data is an investigation that could very well be conducted by Mr. Maginnis—in fact I know it is, if he would find time to do it; we should then have some results, of which the paper is at present altogether bare, and not merely statistics, which are no doubt useful, but a complete paper, teaching the lessons which are taught by the performances of these ships. I hope Mr. Maginnis will find it in the interest of this Institution, and in his own interest, to make these additions to his paper.

Mr. A. J. Maginnis: Mr. Chairman and gentlemen, with reference to Mr. Denny's remarks, the reply that I can make as regards the contents of my paper, in connection with the competition for which it has been sent in, is simply this, that I have been in a position fortunately to obtain actual results, and not those of trial trips, which are never afterwards in actual practice borne out, because on trial trips you have always exceptional circumstances—you have picked stokers, you have picked coal, you have experienced men overlooking your machinery, you have your ship trimmed in a proper condition, and in almost all cases you have almost smooth water. I think the statistics given in my tables may be relied upon, being the actual results; for I think I may say positively, take it how you will, or in what way you like, in connection with steam-ships, if you want to get the true results, and the actual work the steamers are doing, you must get those results from their daily performances. It will not do to take experimental trials for them. Shipowners will not accept from you the result of your trial trips as being what the steamer will do, because in nine cases out of ten—and I think you will all bear me out—afterwards, when the steamer goes on her regular station, she rarely, if ever, comes up to her trial trip performances. Then as regards the statistics which I have given, I have gone into the matter, as much as I know about the propeller, and have endeavoured to give them in a way bearing on its efficiency. For instance, from my knowledge and experience of the steamers named by me—*Alpha*, *Beta*, and *Gamma*—extending over a number of years, I have looked over the logs of the voyages, when the larger propellers were on, and have noted the state of the weather, the draught of water on the voyage, and the length of time the ship was out of dry dock. Having got all these particulars, I then looked over the logs of the voyages when the smaller propellers were on, and picked out voyages made under similar circumstances, and have placed them together, so that the relative merits of the propellers may be more easily seen; the voyages marked A, being those of fine weather passage, and those marked B of a rough weather passage. I have myself gone over those logs figure by figure, and have endeavoured

to make the tables as near as possible correct, especially as to the draught of water, and the time out of dry dock, as these circumstances materially affect the propeller. Of course it is impossible in these results to get the dates within a fortnight or so of each other. As regards what Mr. Flannery said in reference to the extension of my paper, I am very much afraid ship-owners are not inclined to allow such extensive particulars to be made public, as he would like me to add. I have purposely omitted all dimensions of the vessels and engines, so as to keep the tables as simple as possible, and because it seems unnecessary to draw up elaborate tables of particulars, which may be found in the underwriter's books. Unfortunately, I have not been permitted to give the real names of the steamers, but if any gentleman should like further information concerning the vessels, I am at liberty to give him the names. I have throughout the paper endeavoured to show, not my own ideas on the subject, but those which the actual practice of a few years has taught. Most of the facts mentioned in my paper have come directly under my own notice, and I have no hesitation in saying, that no author or writer who has ever written on the subject, or ever will write, would think of, or even expect the strange breakdowns which have occurred, or suggest the improvements which actual experience has taught during the past few years.

ON THE RESISTANCE GIVEN TO SCREW SHIPS BY THE ACTION OF THE SCREW-PROPELLER, AND HOW TO REMEDY IT.

By Robert Griffiths.

[Read at the Twentieth Session of the Institution of Naval Architects, 3rd April, 1879, N. Barnaby, C.B., Vice-President, in the Chair.]

When making a series of experiments with a ship-model of 5 ft. long, 10½ in. beam, propelled by two separate springs of equal strength, and representing a screw ship with separate engines, I discovered that there was considerable suction of the eddy-water caused by the screw at the upper part of its disc. I had a telescopic pipe ¾-in. diameter, which led from a tank inside the model, and which could be pushed out till the end was close to the top part of the screw's disc. When this pipe was pushed out and was plugged at the inner end, the model went 48 ft. in a minute with 700 revolutions of the screw; again, when the plug was removed from the pipe and the water was allowed to flow into the tank, and be drawn out by the suction of the screw, the model went 61 ft. with 700 revolutions, while when the pipe was pushed in the model went 75 ft. with 745 revolutions. When the model was moored the water would flow in through the pipe and fill the tank in 10½ seconds; if the screw was then started it would empty the tank in 10 seconds, and when the model was going the screw would empty the tank in 8 seconds.

I will now refer to some trials made on Messrs. Penn & Sons' ship the *Elephant*, with a four-bladed screw 8 ft. diameter and 11 ft. pitch, the boss being 4 ft. 6 in. diameter; this screw was first tried in a casing, and the speed was 7·836 knots with 239 I.H.P.; the casing was then removed and the speed was 8·296 with 240 I.H.P.; the speed with an ordinary three-bladed screw 8 ft. diameter and 10 ft. pitch was 8·416 knots with 238·6 I.H.P. The *Elephant* has a very full stern, and I was disappointed with the results until I made further experiments, which proved that with a full-stern vessel a casing over the screw and an enlarged centre boss would cause a loss of speed, as the currents from each side of the stern come on to the outside of the casing, and the screw has to get its supply from the dead water in front of it. When the screw is working without a casing it takes a great portion of its supply from outside its disc at the forward edge and as far as the middle of the blades, and draws in the currents towards it, by which it reduces the eddy. With a fine run ship the currents meet nearer the stern and the dead water is much reduced.

The first experiment I made to find out the cause of the unsatisfactory results in the *Elephant* was making the projections on each side of the deadwood which covered the boss

longer, and tapered towards the screw instead of being parallel with the screw-shaft; this caused a loss of 10 per cent. in the speed of the model, and gave an idea of what was required. I then shortened the stern and made the run, as high up as the top of the screw, end $2\frac{1}{2}$ in. in front of where it originally ended; from the end of the run to the sternpost I had a parallel piece of the thickness of the sternpost, and the screw was left in the original position; this of course made the run or wedge-form of the stern fuller. By this alteration the model gained 15 per cent. in speed, the power exerted being the same. I then altered the model to what it was before it was shortened, which reduced the speed to what it originally was. I have since made experiments with four-bladed screws with blades the same width from the boss to the points, and with blades tapered from the boss to the points on the leading edge only, the result being that by lengthening or fining the run there was a slight increase of speed of 3 to 5 per cent.; but by shortening the run and making it fuller, and placing the screw about two-thirds its diameter behind the tapered part, and leaving the intermediate space open, or filling in a parallel piece of the thickness of the sternpost, an increase of speed of 10 to 15 per cent. was obtained, for which I account in the following manner: as the ship moves forward the water closes in from each side of the stern to fill the space she occupied; when the screw is in operation it sucks away the eddy-water at the top part of its disc, which mostly accumulates there, owing to the fulness of the run from the level of the screw shaft to the top of the screw, which water would follow the ship if the screw did not draw it away, by which the thrust given to the screw-shaft is increased, and increases the resistance of the ship to nearly twice as much as it would have been had the screw not been in operation. The only remedy that I have been able to discover is to place the screw as far back from the tapered or wedge-part of the stern as will allow the eddy-water to remain there, for which purpose the screw must be placed about two-thirds its diameter behind the sternpost, so that the currents which flow on each side of the deadwood will be drawn by the action of the screw and meet in front of it, and supply it with the water to act upon.

Fig. 1

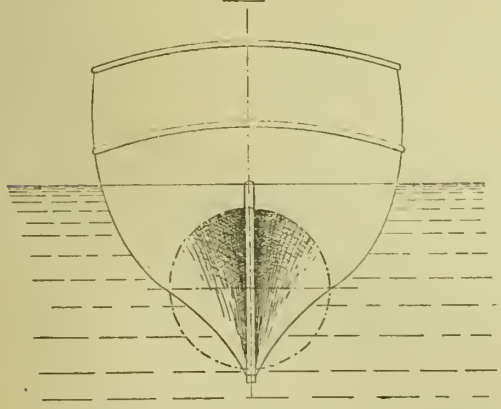
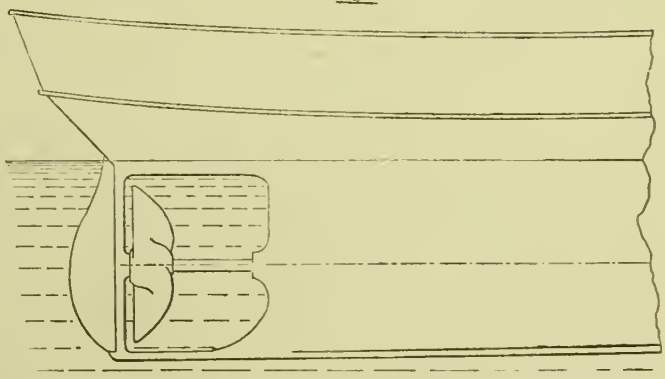


Fig. 2



In order to obtain the full effect of the power that is exerted to propel the vessel, it will be necessary to form the sterns of screw ships as shown in Figs. 1 and 2, unless ship-

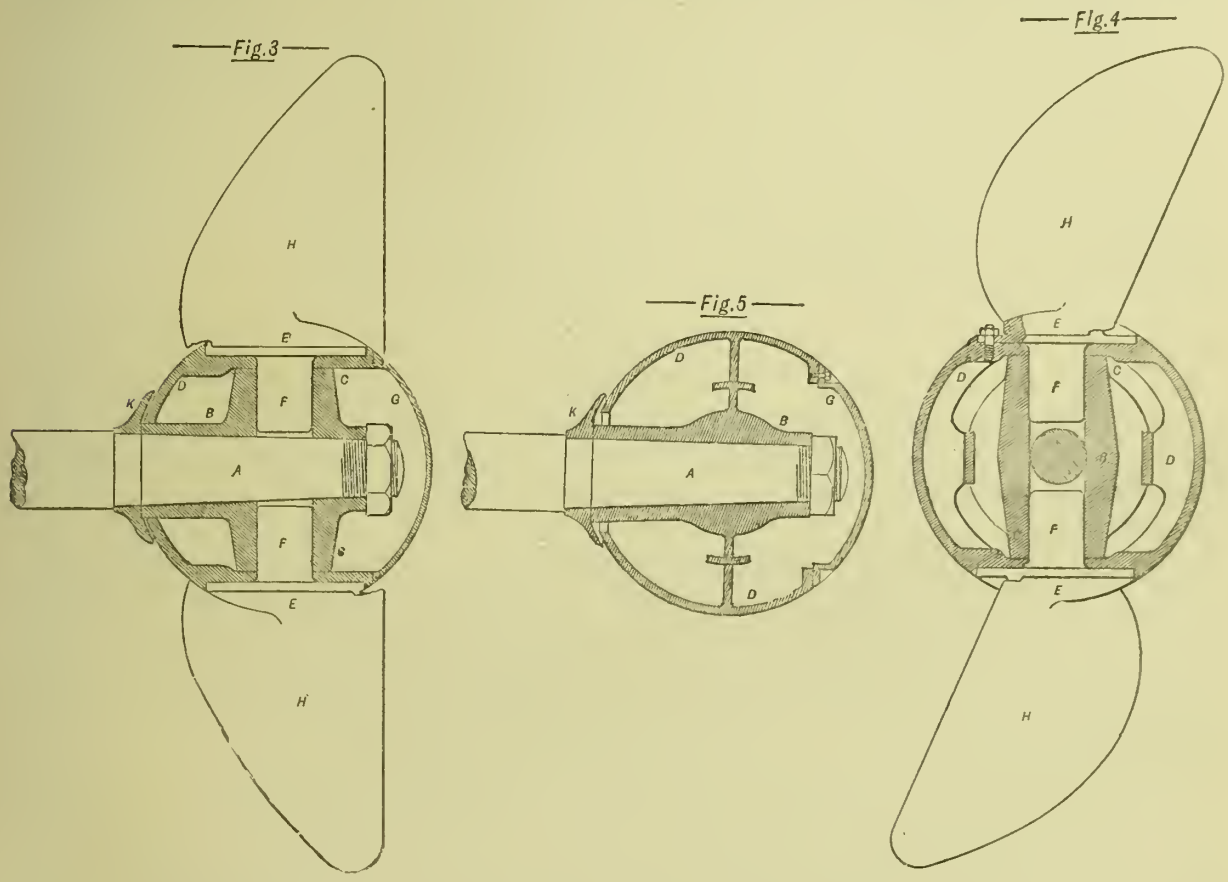
builders can give a valid objection to it; the shortening of the deadwood would reduce the displacement but little, and that could be compensated for by enlarging the counter above the screw, as shown, which would then take a portion of the water which the screw forces back in its wake.

There is one other feature in connection with these experiments which proves the theory of my improvements: a piece of the deadwood 1½ in. long was removed, the screw being left in the usual position; this made no difference in the speed, which was 51 ft. in half a minute with 525 revolutions in 60 seconds. The screw was then moved ½ in. forward, when the speed was reduced to 46 ft. with 525 revolutions; the screw was then moved another ½ in. forward, and the speed was further reduced to 40 ft. with 520 revolutions; and, finally, when the screw was moved a third ½ in. forward, the speed was only 26 ft. with 500 revolutions. The screw with the leading part of the blades tapered off was then tried in the first position, and gave a speed of 54 ft. with 545 revolutions; when moved ½ in. further back the speed was increased to 58 ft. with 550 revolutions; and at an inch back 59 ft. with 550 revolutions; and at 1½ in. back to 60 ft. with 555 revolutions.

There is very little doubt that in full-stern and other ships the deadwater that is formed between the stern and the place where the outside streams meet goes forward with the ship and forms the wedge or taper-end to the deadwood; this does not cause nearly the resistance to sailing ships that it does to screw ships, through the action of the screw sucking it away. Colonel Beaufoy, by his experiments, upwards of half a century since, found that when the taper or wedge-end of his model was at an angle of 6° 22′ 45″ there was no resistance at any speed up to 8 knots, but that when the angle was 19° 28′ 16″ the resistance at 8 knots was 34·34 lbs., and gradually increased from 0·2101 lbs. at a speed of 1 ft. per second to 34·33 lbs. at 13·527 ft. per second, his model being 12 in. square, with tapered wedge-ends.

Screw propellers, when working, act on the currents of water which flow by the stern of the vessel, and by accelerating the velocity of those currents meet with the resistance by which they are enabled to push the ship forward; there is, however, considerable difference in the velocity at which the currents flow at different parts of the screw's disc, and consequently, the screw's blades meet with more resistance in passing over some parts of the disc than others; this of course results in the screw giving greater thrust in some positions, as shown in the dynamometer diagrams of H.M.S. *Rattler*, in which the thrust varied between 2·9 and 4·1 tons in each revolution. Besides indirect proofs from thrust diagrams, direct proof of the difference of the velocities of the currents was obtained from experiments I was allowed to make on H.M. steam pinnace No. 22, at Devonport, in 1875, by measuring, with apparatus specially constructed for the purpose, the rate at which they flowed through the screw's disc while the boat was towed. These experiments showed that over the bottom half of the screw's disc the water was little interfered with, and the water passed through the disc there at nearly the speed at which the boat was towed, but at the top half of the disc the water was dragged with the boat to a certain extent, and only flowed through the disc at about half the speed at which the boat was going. Fig. 1 shows the results, the currents having the least velocity where the shading is greatest.

When a screw is working, the blades as they pass over the top part of the disc meet with more resistance than when they are passing over the bottom part, for as the screw, to obtain the thrust, drives back a column of water the size of the screw's disc, at a velocity corresponding to the power employed, it has to accelerate the velocity more at the top of the disc than at the bottom, and consequently more power is expended on the top part of the disc than on the bottom, and from this cause the increase in the resistance of the ship from the action of the screw, which Mr. Froude has proved is always from forty to fifty per cent. of the total resistance of the ship, is considerably more than if the power were



uniformly expended over the disc, for the ship is much retarded by the drawing away of the dead-water (so to speak) from the top of the disc, whereas accelerating the currents at the bottom of the disc would have hardly any effect on the ship. Again, as the blades one after another pass over the top part of the disc, and as each blade meets with an increase of resistance there, a series of jerks is given to the stern of the vessel, which is the cause of vibration.

Figs. 3, 4, and 5 show a screw so constructed that the blades always meet with the same resistance. On the shaft A is fixed a casting B, with two sockets CC on opposite sides; the shell D of the boss, which can just pass over these sockets, has an elliptical hole

in the forward end, through which the end of the casting B passes, and which allows of a suitable amount of movement. The blades HH, which are formed so that most of their surface is aft of the middle of the boss, are constructed with flanges EE, and shanks FF; these flanges are bolted in the usual way to the shell D of the boss, the shanks slipping into the sockets CC; a cap G completes the shell of the boss, and a cover K fixed on the shaft in front of the boss prevents floating substances entering the boss through the elliptical hole. It is obvious that as the blades have more than half their surfaces aft of the middle of the shanks, the pressure on their surfaces tends to fine their pitch; and also, since they are rigidly connected with the shell D of the boss, that as one blade turns the other is also turned, the pitch of one being increased as that of the other decreases; when, therefore, one blade meets with more resistance than the other, the difference of pressure causes that blade to turn, reducing its pitch and increasing the pitch of the other until the pressure on each is equal. By these means the injurious action of the screw is to a great extent done away with. The cost of construction of this screw would not exceed that of an ordinary screw, and no alteration in the shaft or vessel would be necessary for applying it, and at any time the blades could be fixed and the screw worked as an ordinary screw.

DISCUSSION.

The Chairman: I do not know whether Mr. Griffiths' description of his proposed form of screw has been quite understood. There is a model here which is intended to illustrate it, but if it has been sufficiently understood I shall be glad if the gentlemen present will offer any observations they may have to make.

Mr. Benjamin Martell: I should like to ask Mr. Griffiths what his experience has been in reference to the trunk that he proposed some time ago, and which, I think, he had fitted to some models, where the screw worked in it, and he said that he found a very great benefit was derived from that. He intended, I understood, to carry out those experiments, and stated in a paper read on it here, either last session or the session before, that he hoped considerable benefit would be derived from that. I am rather disappointed that Mr. Griffiths has not alluded to it in this paper, and I should like very much to know what the result of his experiments has been in regard to that.

Mr. Thornycroft: Mr. Chairman and gentlemen, I should like to make one or two remarks on the screw propeller, particularly on the very interesting diagram which Mr. Griffiths has given us, where he refers to the difference of speed of the water through the screw disc, at the upper and lower parts of it. I did not quite catch the figures, but there appeared to me to be a very great difference of speed in the water at the disc in the different parts, and the arrangement which Mr. Griffiths has shown us seems certainly calculated to give some advantage; the only doubt in my mind is whether or not the mechanism can be made so that the alteration of form which occurs in the screw shall not be accompanied by too much wear. There is another thing I have thought about, which this screw bears upon, which is this: that in steering a screw vessel I lately came to the conclusion that there was a new element—at least, new to me—in side resistance. Other people may have thought of it, but when a screw is propelling a vessel rapidly, and there is great power being exerted on the screw blades, any force exerted to move the stern of the vessel sideways is opposed by the screw blades working unequally in different parts of their revolution, and so opposing that force. You alter the position of

the screw disc, where the greatest work is done, if you move the vessel's stern sideways, and I have come to the conclusion that there is very great stability in resisting side motion in the action of a screw propeller. Another curious thing which happens is this, that Mr. Griffiths' propeller, if it works well, is calculated to remove this difficulty of steering in a screw vessel. We have lately had our attention called to an American vessel where this difficulty was got over in a curious way, by taking the screw nearer the middle of the vessel, so that it could not stop the vessel turning. I do not know that I have any other remarks to make.

Professor A. B. W. Kennedy: Mr. Griffiths having alluded to model experiments, I should like to say a word or two on the nature of the experiments, from which we can deduce any results whatever about the screw propeller. I think we may say that there are three methods under which experiments, or what we may call experiments, may be made. We may use the results of ordinary working as have been done by Mr. Maginnis, we may get the results of trial trips, or we may take the results of experiments made with models, and certainly the value of any statements made here, in papers or otherwise, depends very much upon the way in which the results which are stated have been obtained. Now, to take first the trial trips. Of course it is very easy to abuse them, and we all know that trial trips do not always represent the actual doings of the ship afterwards. That, however, is purely a commercial question, it is not a question which concerns the efficiency of any particular propeller at all. The difference between the result on a trial trip and the result of any ordinary voyage, so far as this question of mechanical propulsion is concerned, is simply this: we do really know the results of a trial trip, more or less; we do *not* know the results of the ordinary voyage. I think most marine engineers will bear me out in saying that even a very carefully kept log is a thing which can do no more at the very best than give us something like a rough average of what has been done, but comparing the log of one voyage with the log of another voyage under somewhat similar circumstances, and about the same time of year, and with the same kind of freight, is not making an experiment, it is nothing more than giving us a rough comparison. A trial trip is no doubt carried on with picked men, and under very special conditions; an experiment is always made under special conditions. In such an experiment, we are not only justified in picking our men and choosing our conditions, but it is only by doing so that we can know what is actually done on the trial trip. I claim for trial trips that they do give us some results which we can reasonably trust. Whether they are results which the ship will attain afterwards or not is quite a different question and one which concerns the owners of the ship chiefly, but as marine engineers and naval architects, here we are concerned with having results stated accurately, and I do maintain that we get this far better from trial trips than by averaging the results of ordinary working. As to the experiments made from models, we know from the results that Mr. Froude has given, it is possible to obtain results from model experiments which can be applied to actual working, but we also know just as well as that, from the results of Mr. Froude's experiments, that it is impossible to apply the results obtained from models directly to full-sized ships. You must first of all direct your investigation to the comparison of the relations between the models and the ships, so that it is just as easy to make a wrong experiment with models, under certain circumstances, as not. It is no use trying experiments with models and saying that they will tell us about ships, because Mr. Froude's experiments tell us about ships, unless we can also say that we have investigated the relations of our model to the actual full-sized work, as Mr. Froude has done through these very many years of actual careful work. I do not think in saying this that I have at all misunderstood what Mr. Froude has often said as to the relation between his experiments and actual work. I can only say, as having had something to do with experimental work at various times, that really the difficulty of deducing accurate results—results that one can apply scientifically, from the mass of information you get out of even a well-kept log

book, is exceedingly great, and that even in the hands of very careful engineers those results must be taken as not representing very much more than averages.

Mr. C. W. Merrifield: If Mr. Kennedy had not done it, I intended to have called attention, in much the same language as he has done, to the importance of knowing, when you are comparing the performance of models with the performance of full-sized ships, accurately the law which connects the performance of the model with the performance of the ship. That is an absolutely necessary complement to the inference of arriving at the performance of the one from the performance of the other. I was particularly desirous to emphasise this, because mistakes are so often made about it, and, in particular, it has sometimes been said that I was wrong in my difference with Mr. Froude, at the time I wrote the report to the British Association of the performance of ships, in that I did not consider that any experiment with models was then of any considerable value at all. Mr. Froude has since removed the difficulties that then lay in our way, by trying experiments on full-sized ships, and verifying experimentally the law which was then a mere assumption. I think it was first stated by Professor Reech, and I believe was also originally, quite independently, reinvented by Mr. Froude, that in comparing the propulsion of ships and models, the speeds of the ships ought to vary as the squares of their lineal dimensions, but, at the time I wrote that report, this was a mere theoretical belief, and was not experimentally verified. It was not until the *Greyhound* experiments were tried on a large scale by Mr. Froude, simultaneously with that splendid series of experiments tried at Chelston Cross, that that was even approximately verified, and in attempting to extend that law to other details of propulsion, beyond the general sort of average of propulsion which you get on comparing the whole speed of one vessel with the whole speed of another, I think you introduce new terms, which may or may not be fitted by the direct formula connecting the average performance of the vessels. I think it is very desirable it should be borne distinctly in mind, in comparing the results of models and experiments, that you require not only to assume the law, but to establish it, and to show how it fits every detail to which it has to be applied.

Mr. William Denny: Had Professor Kennedy not risen to correct the impression which seems to pervade certain minds, that the more complicated you can get the conditions of an experiment the more valuable that experiment is, I would have risen to explain it myself, and I regret exceedingly to find that a love of rule of thumb, of what is supposed to be the practical man's condition of mind, is apt to lead away so many gentlemen from what is clearly the practical condition of mind, namely, to eliminate from your experiment every undecided quantity, and to place it upon a clear basis. I think it is lamentable that this condition of mind should exist, and I think that the sooner gentlemen come round to see that the really practical men who are dealing with the work of the day, are Mr. Froude and the gentlemen representing the Admiralty, and those who are carrying out experiments, and who have attempted, through good report and through evil report, to fight this impression of the practical working mind, and to eliminate all doubtful facts, especially such facts as occur in the voyage of a ship, the better it will be. Professor Kennedy pointed this out so ably, that I am perfectly convinced that the body of the members of this Institution have clear enough heads to see the thing. I think we can hardly pass over Mr. Griffiths' paper without expressing our great pleasure in having, from a veteran in screw propellers, the paper which he has put before us. Anyone would be inclined to criticise such a paper liberally and generously as coming from him, and I think he deserves the greatest credit for what he has done. He has come forward and tried to put Mr. Froude's theories into actual fact. No ship-builder has yet tried to do it, and I think we owe Mr. Griffiths an acknowledgment for what he has done, and for what he has pointed out. The only thing I should like to ask Mr. Griffiths about is this: In the experiments which I believe he carried out with these models he gives us the speed, he gives us the

results, but there is the one difficulty, what was the power exerted on the model? Well, as far as I have verified his experiments, I have not been able to find out what that power is. I do not think the paper has distinctly and clearly given us the power that is acting at the other end of the shaft, and therefore it is quite impossible to say what the merit of the propeller is.

Mr. A. C. Kirk: I was very pleased indeed to find Professor Kennedy speaking so strongly and favourably of trial-trip experiments as giving us the clear relation between the power and speed of ships. I have analysed a great many voyages of steamers of various companies, and I must confess it has resulted in conparatively little information indeed. Now, trial-trip experiments will not certainly give you the absolute speed of a ship at sea. A ship at sea never maintains her trial speed, as I think most of us know, but you get the correct relation between the power and the speed, and the speed at sea is simply a question of the power of continuance of an engine—what power you can keep up at sea—which is a very simple affair. Engines will generally keep up at sea, if decently well managed, three-fourths of what they will do if properly worked upon a trial trip. In other cases, if they are decently managed they will do about two-thirds. Much depends on the qualities of the coals supplied, but it runs between those two figures. As to the relation between the horse-power, and the consumption of coal, and the speed, I have not the slightest faith in logs, but I have very strong faith in well-conducted trial-trip experiments, but, as I have said, they must be properly and well conducted.

Mr. Ravenhill: I feel I must make a remark or two after what has passed from our friends here to-night from the Clyde. I remember the time when trial trips were almost confined to the neighbourhood of London. I believe the only measured mile was the mile on the Thames, and it is gratifying to find that our friends now lay such stress on the experiments made on the measured mile. We have been to-night over the old ground, because there are two sides to the question. If you look at it on the scientific side, there can be no doubt that the measured-mile trips are most valuable. The measured-mile trials as recorded by the Admiralty, the trials as recorded by the Peninsular and Oriental Company, and possibly others, are most valuable tables of reference; but, unfortunately, a shipowner cares but little about them. We have that battle to fight with him. You all know perfectly well that the moment a ship is ready for a trial trip, the owner wants to send her out to sea, and to begin to earn some return for the capital which he has invested. Once that is done he only looks to the carrying powers of his ship, and the cost of coal entailed on him to convey so many tons of goods from one port to another. Discussions like this may so far do good as eventually to bring shipowners to give a little more time to experiments such as we should all desire to see made with every steamship before she permanently takes up her station. If a series of experiments with all the ships that are built could only be carried out for say a year or a year and a half, the amount of information we should in that time collect would be most invaluable data for the future. I do not know how we as an Institution can put that prominently before them, but if it could be devised in any way it would be, I am sure, attended with most beneficial results to all of us. We evidently have a great deal to learn.

Mr. Edward James Harland: My Lord and gentlemen, I cannot refrain from giving a decided expression on my part against the practice of short distance trips, because the amount of jockeying and dodging that goes on in running a measured mile has been a greater stumblingblock to progress than anything else. I am perfectly satisfied that more sound constitutions in the structure of marine engines are destroyed and ruined for their lives, by measured-mile trips than by fair working at sea, and I have regretted exceedingly sometimes seeing the absurd, I may say ridiculous, amount of indicated horse-power that has been actually, no doubt by figures, given out at the trial trips of some of our magnificent frigates, when I am satisfied it would

not be safe ever again to attempt it, for one reason, and that is that such a power is never required to be developed, and next, because the parts of the machinery have become so injured or so strained by the extraordinary development of power in the measured mile, that it would be extremely unwise to attempt it again. Therefore, on behalf of the shipowners, although Professor Kennedy as much as said, "Let the shipowners look to their part of the business: as professional men, it is for us to see that we get out certain nominal results," I say it is our business as naval architects to consider the interest of shipowners, because it is on them we have to rely entirely. If we cannot produce that which is practically of use to the shipowner, our vocation, or our profession, if you choose to give it that name, is entirely gone. Therefore, on behalf of the shipowners, I say that trial trips over the measured mile are simple deceptions, and if I were a shipowner I would not allow anything of the sort, and as ship-builders we allow nothing of the sort. When we make a contract for a ship to be finished by us, our contract is that the ship shall give out that work continuously—one hour, six hours, or sixty hours. The boilers are so arranged that the feed can be on during the whole of the time. Now, we know perfectly well how dodging goes on in the feed in trial-trips over a measured mile. We know the accumulation of pressure, and all those series of nonsensical matters which I think, as naval architects and engineers, we ought to be entirely above. If our engines are fit to do their work commercially, for that is the real result of the whole thing, they should be able to do it continuously, and not under circumstances only of special stokers picked coal, and all sorts of nonsense which, in the early days of naval architecture, were very fine, but we have now to deal with facts, and in foreign competition all that sort of thing is passed. I would strongly urge that we do not allow ourselves to be any longer bamboozled by anything of the kind. I am extremely glad to find that the Admiralty have most wisely, latterly, changed that condition of things, and I am delighted to hear that on a recent occasion of a trial trip it was for six hours. I say that is a very decent time. You can have it in the middle of the day and pick your ground, and if the ship will do it for six hours she will do it for sixty, but as for running a paltry mile, your big ship has hardly time to get under way before she is turned round and the whole thing is over. We have the pockets of the shipowners to consider; the Government is another matter, and I leave it for them to consider; but as far as the mercantile marine is concerned, it is really, after all, what will the ship do for six hours or sixty hours?

Mr. Parker: Mr. Chairman, I am sorry to see our friends from the Clyde on the left, and our friend Mr. Harland, on the right so much disagree about these trial trips. I am also sorry to hear that Mr. Ravenhill thinks the shipowners do not interest themselves nor care much about trial trips being carried on correctly. It may be interesting to this meeting to know that two large firms in Liverpool have arranged with the captains and engineers of their steamers, to make correct progressive trials varying from four to the maximum number of knots the vessels are capable of making while on their voyage to India. I had sent to me yesterday a very nice table, the result of one ship's performance; her length is 361 ft., beam 37 ft., depth 26·3 ft., and she was loaded to a draft of 21 ft. 9 in.; the speed, power, and consumption per hour is given progressing from 5 knots to 13 knots. I have also obtained permission from a very large steam shipowner in London to arrange with his engineers and captains while their vessels are on their voyages to make trials of a similar nature. The result of these trials I shall tabulate, and some day I hope to be able to lay them before this Institution.

Mr. H. S. Mackenzie: I am a stranger here, but if allowed to speak I should be glad to return my thanks to the gentleman before Mr. Parker for taking the side of the shipowners. I agree with him in a great many points, and I can simply say that I never go by any trial trips, and the trial trips of vessels in which I am interested have always been when laden. They have to run a certain distance

along the coast in the direction in which they have to go when deeply laden. If the owners are there, and find the vessel does the speed which the engineers and builders guaranteed she would do fully laden during five or six hours, we feel perfectly satisfied, and the vessels generally have done what the builders and engineers said they would do. I rise also to say a few words in confirmation of what Mr. Griffiths has said. I happen to be the largest private experimentaliser in screws in this country. I never had any assistance from Government or from any Association, and I have spent thousands of pounds in carrying on experiments with screws, and I can quite verify what Mr. Griffiths has said. I have a screw yacht of between 40 and 50 tons, and by simply moving the screw in the way Mr. Griffiths has mentioned, I attained very favourable results. I fancy I read an account of it some six weeks since in the engineering papers, and it tallied exactly with my own experience. The further the screw is moved in a space such as is described in that drawing, the better would be result. But our large merchant ships or men-of-war would have some difficulty with regard to the construction, because it would weaken the vessel to cut out so much. It simply comes to this, that the further you move the screw from the body of the vessel the better, and the gain of one knot in the yacht to which I have referred was made by moving the screw only nine inches. It was possible to move it 21 inches, the space in the vessel being very large, owing to her peculiar arrangement. I believe the first gentleman that ever adapted that to screw propulsion was Mr. Erichsen, and although there might be other claimants, yet I think I am right in giving him the credit of it. He drives his screw in halves, and each screw, right and left-handed, worked in opposite directions on a concentric shaft. Those experiments were carried out to a result which I believe has been attained in this very yacht which I referred to. But on taking off one of those screws, the right-handed one, leaving the left-handed one on, it was close up to the sternpost. The vessel's speed was 9½ knots, with her engines working at their ordinary revolutions, at about 120, with a pressure of 75 on the boiler—no other pressure, no overworking in the engine-room or anything. That is very fair work, 9½ knots with 120 revolutions; but with a screw five feet in diameter and eight feet pitch, simply moved 9½ inches further away from the body of the vessel, she went 10½ knots as nearly as possible all the way from Falmouth to Dover and back again continously. I should be very happy to have Mr. Griffiths or any member, when that vessel is in the Thames again, to try that with any form of screw; and I feel confident (it does not matter what form of screw), by moving it in that space as mentioned by Mr. Griffiths, that result will be gained. Whether it is done by having an open space or by having the screw further away from the vessel, I cannot tell, but it so happens it is an open space in my vessel. I mention that merely to encourage Mr. Griffiths in what he is doing. Having carried out Mr. Froude's calculations on a small scale, I have verified them on a large one with a vessel of 50 tons, with a screw five feet in diameter, with eight feet pitch, with a revolution of 120; and I have increased the speed of that vessel nearly one knot by a movement of the propeller of nine inches further away from the vessel.

Mr. R. Griffiths: The first question that was asked was with reference to the trunk. I tried an experiment on a ship of Mr. Penn's, the *Elephant*—a very full-sterned vessel. I put a screw with a boss nearly two-thirds of its diameter and the same pitch as the old three-bladed common screw in a trunk, and on the stern I fitted parallel cylindrical pieces of the same diameter as the boss, to prevent the water coming against the front of the boss. By this arrangement, to my great disappointment, I lost half a knot. I then had the trunk taken off and left the screw as it was, and the speed was then, within a mere trifle, the same as with Mr. Penn's old screw, the three-bladed one. After that I determined to find out the cause of the loss of speed. I first altered my model to the form of the *Elephant*, and then found that I obtained similar results from it, to those I had already obtained from the *Elephant*. I next substituted conical pieces for the cylindrical ones in front of the boss, and this resulted in a loss. This first drew my attention to the fact that whenever a screw is near the

taper of the hull of a ship the action of the screw in drawing away the water from the tapered surface increases the resistance of the ship, and in order to test this idea more fully I tried various screws, placing them first near the end of the run and then moving them further away gradually. By this I found that in every case the speed increased as the screw was moved aft, until it was two-thirds of its diameter from the end of the run, and there the speed was a maximum, being about fifteen to twenty per cent. more than when the screw was in the usual position, and that as the screw was moved still further back the speed gradually decreased. These experiments were made with various amounts of power, the maximum being 50 ft. pounds per minute, and as the weight of my model was 65 lbs., and the speed then about 120 feet per minute, the power divided by the speed was about one two-hundredth of the weight of the model, which is about the proportion which exists in our large ironclads. This is so simple that I have no doubt in a very few months it will be proved on a large scale. I am now prepared to put a screw on any suitable ship at my own cost, if the owners will give me half the value of the engine power that would be required to obtain the increase of speed that my improvements will effect.

PROCEEDINGS ON APRIL 4TH.

A Special General Meeting of Members and Associates was held on Friday, April 4th, in the Hall of the Society of Arts, at 11 a.m., LORD HAMPTON in the chair, when the following alterations and new bye-laws proposed by the Council were agreed to unanimously, on the motion of Mr. N. BARNABY, seconded by Mr. A. C. KIRK :—

7. After the words "shall be held annually" *insert* "before Easter in each year."

30. In January of each year the Council shall meet and prepare Lists for the Election of the Council for the ensuing year. These Lists shall be as follows, namely :—

1st. A List of the names of the President, Vice-Presidents, and Treasurer, for the ensuing year, to be submitted at the Annual General Meeting, for their election in a body.

2nd. Lists for the Election of the Ordinary Members and Associate Members of Council. This List shall contain the names of the existing Ordinary Members of Council, and Associate Members of Council, and at least six new names of Members, and at least two new names of Associate Members of the Institution.

31. No addition shall be made to the total number of Ordinary Members of the Council until by death or resignation, their numbers shall have been reduced below twenty-four, after which their numbers shall be raised to and preserved at twenty-four. And no addition shall be made to the Associate Members of Council until, by death or resignation, their numbers shall have been reduced below eight, after which their numbers shall be raised to and preserved at eight, always exclusive of the President, Vice-Presidents, and Treasurer.

32. Not later than the 14th February in each year, the complete Lists proposed by the Council for the Ordinary Council for the ensuing year shall be printed, and sent to all Members to serve as Balloting Lists. These Lists shall contain, first, the names of the existing Ordinary Members of Council at the time of the preparation of the Balloting List, together with six new names of Members of the Institution (or, if the number shall have been reduced below twenty-four, then so many new names as shall be needed to bring the number up to thirty), and the whole of these names shall be printed in alphabetical order without any distinction of type. Secondly, the names of the existing Associate Members of Council at the time of the preparation of the Balloting List, together with two new names of Associates of the Institution (or, if the number shall have been reduced below eight, then so many new names as shall be needed to bring the number up to ten), and these names also shall be printed in alphabetical order without distinction of type. From these Lists the new Council shall be elected. Every Member shall be at liberty to strike out any name on the Lists, but he must strike out SIX names from the Lists of Members of Council, and TWO names from the List of Associate Members of Council.

33. A similar Balloting List (in which, however, the names of the ordinary Members of Council proposed for Election shall not be included) shall be printed and sent to all Associates of the Institution, to serve as a Balloting List for Associates, from which the voting for Associate Members of Council shall be taken. Every Associate shall be at liberty to strike out any names on that List;

but he *must* reduce the number by striking out *two* names from the List of Associate Members of Council.

34. The Balloting Lists may be sent by post or otherwise to the Secretary, so as to reach him before the day and hour named for the Annual General Meeting, or they may be personally presented by the Members and Associates at the opening of the Annual General Meeting.

35. At the opening of the Annual General Meeting the order of business shall be :—

(1) To read and consider the Reports of the Council and Treasurer.

(2) To read the List of Officers and Nomination for Council for the ensuing year, proposed by the Council.

(3) The Chairman shall next put to the Meeting the List containing the names of the President, Vice-Presidents, and Treasurer, for election for the ensuing year.

(4) The Chairman shall then nominate two Scrutineers (of whom one only shall be a Member of the existing or proposed Council), and shall hand to them the Ballot Boxes containing the Voting Papers for the Ordinary Members of Council and Associate Members of Council, and

(5) The Scrutineers, assisted by the Secretary and his clerks, shall receive all Ballot Papers which may have reached him, and all others which may be presented by Members or Associates at the Meeting. The Scrutineers shall then retire, and verify the Lists, and count the votes ; and shall, not later than the following day, report to the Chairman the names which have obtained the greatest number of votes, subject to the conditions of the Ballot. The Chairman shall then read the List presented by the Scrutineers, and shall declare the gentlemen named in the List to be duly elected.

(6) After the Ballot shall have been taken, and the Scrutineers have retired, the Meeting will proceed to the other business before it.

36. The new Council and officers shall take office immediately after the close of the Annual General Meeting.

Memorandum.—Following Bye-Laws to be renumbered.

THE STEAM TRIALS OF H.M.S. "IRIS."

By J. Wright, Esq., Vice-President.

[Paper read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

In the paper on the comparative efficiency of twin screw propellers in deep draught ships, read by Mr. White at last year's meeting of the Institution, some particulars were given of the first trials of H.M. despatch vessel *Iris*, and in the discussion which followed a promise was given to communicate to the Institution the results of some further trials, intended to be made with her, with screw propellers of modified forms and dimensions. I now propose to redeem that promise, so far as I am able to do so.

The trials were not so complete and extensive as it was then hoped that they would have been, for the recasting of the original screw blades and the construction of a pair of new two-bladed screws occupied a considerable length of time, and as the ship had to be completed ready for service by a given date, the proposed series of trials had to be curtailed. However, the results of the trials which were made are interesting and instructive, and will, it is hoped, assist somewhat in throwing light on the hitherto little investigated subject, the application of twin screws to fast steam ships.

I have appended to the paper a list of the principal dimensions of the *Iris*, and her engines and boilers, but as the chief subject of this paper is the performance of the screw propellers, I will not take up time by reading these particulars.

The first official trial, made on the 14th December, 1877, was what is known as the contractors' trial, of six hours' duration, at full power, at sea, to test the efficiency of the machinery generally. The following are the particulars of this trial:—

Draught of Water	Forward	15 ft. 8 in.
	Aft	20 ,, 7 ,,
Area of Midship Section		702 sq. ft.
Displacement		3,300 tons.
Mean Pressure in Cylinders	High	41·5 lbs.
	Low	11·9 ,,
Average Revolutions per Minute		90·318
Average indicated H.P.		7,060
Speed of Ship, about		16·4 knots

On this trial, and on a preliminary trial made just before, it was seen that the speed of the ship was about a knot an hour below what was expected by her designer, the Director of Naval Construction, whose estimate of the speed was confirmed by the result obtained in Mr. Froude's experiments with her model, and it was determined, as had been done from time to time before, to make several series of progressive measured mile trials, to ascertain her performance correctly, and from the results to try and find out the cause of the deficiency of speed.

I can testify, from personal attendance at the trials, that they were all most carefully made by the experienced officers at Portsmouth, but you will, perhaps, find in analysing the results of the trials at the lowest speeds, that in spite of all the care taken some discrepancies appear. These probably have arisen from the difficulty of obtaining the indicated H.P. with sufficient accuracy, and from some variation in the amount of the friction of the engines on the several days of the trials. The two pairs of engines have eight cylinders, four high and four low pressure; at low powers the indicated pressures are very small, and the increments of error which may arise from the friction of the indicators and the measurement of the diagrams, where the top and bottom lines are very close together, will tell much more than at the higher powers.

The hull of the *Iris*, as you are aware, is of Landore steel, and the bottom was coated with Sims's composition. The state of the hull, both above and below water, was, as nearly as possible the same on all the measured mile trials, and they were all made at about the same draught of water—viz. 15 ft. 8 in. forward, and 20 ft. 6 in. aft. Fortunately also the force of the wind and the state of the sea were so nearly similar on the days of the trials that these influences on speed may be left out of account in considering the results of the trials.

At the above draught of water the midship section is 700 square feet; the displacement, 3,290 tons; and the wetted surface, 18,600 square feet.

The original screws of the *Iris* were four-bladed, as shown in Figs. 1, 4, and 7, Plates VI. and VII.; diameter, 18 ft. 6½ in.; the pitch at the leading edge was 17 ft. 2 in., at the after edge 19 ft. 2 in., and the mean pitch, as measured, 18 ft. 2 in. The disc area of the blades is about the usual proportion for four-bladed screws, viz., $\frac{3}{10}$ths of the whole disc. This type of screw had given very good results in a number of single-screw ships, and notably in the *Himalaya*, where it gave the best results of any she has had during her long career. The case of the *Iris* shows, however, that a propeller which has proved very efficient for a single-screw ship may not be well suited for a twin-screw ship.

The particulars of the speed trials with the original screws are given in the Table under First Series, and the indicated H.P. curve is shown on Fig. 10, Plate VIII. It will be observed that the highest speed obtained with 7,503 I.H.P. was only 16·577 knots, or about a knot below what was expected. These screws will be referred to as No. 1.

The second series of progressive trials was made with the same screws, with two of the blades of each removed, but no alteration was made in the pitch of the blades left in place. This was considered to be the simplest and readiest method of proving, as was supposed, that the blade area of the screws with four blades was largely in excess.

The particulars of these trials are given in the Table under Second Series, and

the I.H.P. curve, Fig. 10, Plate VIII., shows a great improvement in the performance as compared with the previous trials. It was not considered desirable to put a very high power on these two-bladed screws, or to run the engines at more than about 90 revolutions, and, consequently, the highest I.H.P. developed was 4,368, with 88·89 revolutions, and the speed obtained was 15·726 knots.

Although this series of trials proved that the blade area of the No. 1 screws was much too large, it could not be accepted as proving that two-bladed screws were better than four-bladed, and as it was desired to retain four blades for the working screws, on account of the less amount of vibration, and of the greater uniformity of action to be obtained in rough weather, the original blades were recast by Messrs. Maudslay, and made to the form and dimensions shown in Figs. 2, 5, and 8, Plates VI. and VII.

The reduced diameter is 16 ft. 3½ in., or 2 ft. 3 in. less than before, but, as will be seen on comparing the forms of the old and new blades, the effective diameter is reduced in a rather larger ratio than the actual diameter. The pitch at the forward edge of the blades is 18 ft. 11½ in., at the after edge 20 ft. 11½ in., and the mean pitch, as measured, is 19 ft. 11½ in., or an increase of 1 ft. 9½ in. on the No. 1 screws. The disc area of the blades is ·288 of the whole disc. The blades are curved aft a little towards the tips, with the view of keeping the points rather further away from the A brackets, and of checking, in some degree, any centrifugal tendency of the water acted on. The blades were polished on both sides to reduce friction, and the edges were made sharp. The original bosses to which the new blades were attached had each a conical tail-piece added. These screws will be referred to as No. 3.

The particulars of the trials are given in the Table under Third Series, and the I.H.P. curve is shown in Fig. 10, Plate VIII. This shows an improvement on No. 2 screws with two blades, and, as compared with No. 1 screws, an increase at full speed of 2 knots, with only 211 I.H.P. more. The maximum speed was 18·57 knots, or about a knot more than the original estimated speed. The vibration was very moderate, considering the large amount of power put through the screws, and the fineness of the stern of the ship.

As the full power of the engines could not be exerted on the No. 2 screws with two blades, a pair of full strength two-bladed screws of the usual well-known Griffiths' shape were made. The blades were polished on both sides, like those of No. 3 screws, and the edges were made sharp; the boss was spherical, and had a conical tail-piece. The diameter is 18 ft. 1½ in., the pitch at the leading edge 20 ft. 3¼ in., at the after edge 22 ft. 3¼ in., and the mean pitch as measured 21 ft. 3¼ in. The disc area of the blades is ·19 of the whole disc. These screws will be referred to as No. 4. They are shown in Figs. 3, 6, and 9, Plates VI. and VII.

The particulars of this series of trials are given in the Table under Fourth Series, and the I.H.P. curve, the best of all, is shown in Fig. 10, Plate VIII. The highest speed given by these screws was 18·587 knots, with 7,556 I.H.P.

The vibration set up in the vessel with these screws in use was greater at all speeds than with either of the four-bladed screws. At the 12½ knots speed the vibration was very moderate, but it increased greatly up to the 15¾ knots speed, and from this it decreased again until the maximum speed of 18½ knots was reached, when it was nothing unusual. After this series of

trials was made it was determined to retain the No. 3 screws for use on the vessel, although their performance on the measured mile was not quite so good. It was intended to reduce the diameter and increase the pitch of No. 3 and No. 4 screws, and try them again, but time did not permit of this being done. The mean pitch at which each screw was set was determined by the revolutions the engines were required to make to develop at least the specified I.H.P., 7,000. The engines being compound, it was necessary to run them at fully 90 revolutions to get them to take enough steam of 60 lbs. pressure to produce the specified power.

You will observe that the blades of all the screws were made with a small increase of pitch, 2 ft. on the width of the blade, with the view of diminishing shock at the leading edge. All the screws were made of gun-metal, as usual in the Navy.

It had been suggested that the engines of the *Iris*, having four high and four low pressure cylinders with the accompanying large number of moving parts, might absorb more power than usual in dead load friction. In order to test this by actual trials, the screw shafting was disconnected at the first coupling, and the engines run at various speeds from 18 up to 90 revolutions per minute. At the low speeds the top and bottom lines of the indicator diagrams, taken with the weakest springs available at the time, were so close together that the mean pressure could not be obtained with sufficient accuracy to warrant me in giving the results as reliable. At the highest speeds, however, I believe the diagrams may be taken as fairly trustworthy.

Each set of engines was kept running steadily while two sets of diagrams were taken. The port engines running at 87·5 revolutions per minute gave 202 I.H.P., and the starboard engines at 90·5 revolutions gave 196 I.H.P., or together 398 I.H.P., in dead load friction. The trials at all speeds showed that the starboard engines worked with the least friction.

In Fig. 11, Plate VIII., are shown the indicated thrust curves for the measured mile trials with the several screws. The indicated thrusts have been calculated by

$$\frac{\text{I.H.P.} \times 33{,}000}{\text{mean pitch of screw} \times \text{revs. per min.}} = \text{Thrust.}$$

The base line shows the speed in knots, and the ordinates the thrust in tons. The curves have been completed in the manner devised by Mr. Froude, and it will be seen that the curve for No. 1 screws shows a dead load friction for the machinery equivalent to an indicated thrust of 5·3 tons.

Now, as before stated, the indicated H.P. of the two sets of engines, running disconnected from the screw shafting, was 398, and this converted into equivalent thrust for No. 1 screws gives 3·78 tons. When this is deducted from the 5·3 tons shown by the curve there is left 1·52 tons, as the amount due to the friction of the screw shafting. From this we find that the horse-power to overcome the resistance, or initial friction of the screw shafting alone at 90·5 revolutions, would be 170. The aggregate initial friction of the engines alone at 90·5 revolutions absorbs practically 400 H.P., and the sum for the engines and shafting 400 + 170 = 570 H.P. The developed I.H.P. with the same speed of engines and the original screws would amount to 7,330, and therefore the power expended on dead load friction was only about eight per cent. of the

gross power. This shows that the engines were so well made and the bearings so well adjusted that they worked with unusually little friction. They compare well in this respect with the machinery of those single screw vessels of the Mercantile Marine, the progressive trials of which were analysed by Mr. Froude and explained at the meeting of 1876. In these he found the average value of this loss to be about thirteen per cent. of the I.H.P., and he assigns this as the probably general value.

I come now to the question why was the performance of No. 1 screws with four blades so much inferior to the performances of all the others?

Authorities on the subject of screw propulsion teach us that the effective thrust or efficiency of a screw propeller depends upon the quantity of water acted on in a given time, and the sternward velocity impressed upon it, and that it is always preferable to make the sternward velocity of the current small, by adopting a form of screw which will act on the largest possible quantities of water. Indeed, it has been said that if it were not for the objectionable element "surface friction," there would theoretically be no objection to an indefinite extension of blade area. In short, it has been always considered as essential to the efficient performance of screws generally, that their blade area should be large, and that the water must be permitted to flow freely to them.

Now, No. 1 screws with four blades *appear* to fulfil the above conditions very completely; for the blade area is undoubtedly very large, and as the run of the *Iris* is very fine, there is no obstruction to an ample supply of water for the screws. Notwithstanding this, however, the results of the trials appear to show that on account of the large diameter of the screws, the shape, and the large area of the blades, combined with a fine pitch and consequent high rotary velocity, an extraordinarily large amount of power was absorbed in the surface friction of the blades. But seeing what a large amount of power appears to have been wasted at the highest speeds with No. 1 screws, it is difficult to believe that it can be all credited to this cause, and it is interesting to form an estimate of the amount absorbed in the surface friction of the blades. In our present state of knowledge it is difficult to determine this with even approximate accuracy, but the elaborate and valuable experiments made by Mr. Froude, in 1872, on the friction of planes, with surfaces of various kinds, travelling edgeways through water, furnish data which at all events enable us to form an estimate of the friction of screw blades, under certain conditions and with certain reasonable assumptions. The following are the results of the calculations which have been made:—

For Nos. 1 and 2 screws, which were coated with Sims's composition, the co-efficient of friction has been taken as a mean of those given by Mr. Froude for a surface of varnish, and one of fine sand. For a mean length of surface of $3\frac{1}{2}$ ft., the width of the blades, this amounts to ·00564, and adding ten per cent. for edge resistance the co-efficient becomes ·0062 for these screws.

The polished surfaces of No. 3 screws were probably less smooth than Mr. Froude's tinfoil surface, and about intermediate between it and the varnished surface. For a mean width of blade of 3 ft. the co-efficient for this screw with 10 per cent. added as before would be ·0038.

No. 4 screws had a similarly smooth surface, but the mean width of blade being 4 ft. the co-efficient becomes ·00365 inclusive of 10 per cent. for edge resistance.

Time will not admit of the details of the calculations being given, but the results showing the horse power* expended on blade friction are as follows:—

No. 1 Screws.

At 91·04	revolutions	per	minute	=	1,120	horse	power.
,, 82·148	,,	,,	,,	=	820	,,	,,
,, 65·105	,,	,,	,,	=	410	,,	,,
,, 43·358	,,	,,	,,	=	120	,,	,,

No. 2 Screws.

At 88·89	revolutions	per	minute	=	530	horse	power.
,, 81·2	,,	,,	,,	=	400	,,	,,
,, 65·06	,,	,,	,,	=	210	,,	,,
,, 45·07	,,	,,	,,	=	70	,,	,,

No. 3 Screws.

At 97·189	revolutions	per	minute	=	420	horse	power.
,, 85·388	,,	,,	,,	=	280	,,	,,
,, 61·343	,,	,,	,,	=	110	,,	,,
,, 40·963	,,	,,	,,	=	30	,,	,,

No. 4 Screws.

At 93·25	revolutions	per	minute	=	330	horse	power.
,, 76·93	,,	,,	,,	=	200	,,	,,
,, 59·385	,,	,,	,,	=	90	,,	,,
,, 39·15	,,	,,	,,	=	30	,,	,,

To admit of the results being more clearly apprehended, curves are given in Fig. 12, Plate VIII., showing the calculated surface friction for the several screws at the various speeds of the progressive trials. The base line gives the revolutions per minute, and the ordinates show the indicated horse-power absorbed in the surface friction of the blades, obtained as explained above.

* It must be remembered that the powers above given as actually expended at the screws on blade friction, are less than the indicated horse-powers which must necessarily be developed, at the engines to overcome these resistances. As the proportion in which the indicated horse-power exceeds the power actually expended on friction at the screws is not obtainable, no estimate of it has been attempted in the paper.

In Fig. 13, Plate VIII., are drawn the horse-power curves for the trials with the several screws, but in this case the power expended in the friction of the screw-blades, as obtained from the above calculations, has been deducted from the total indicated horse-power as developed.

It will be seen at a glance that the performance of No. 1 screws is still much the worst, and that on the above assumptions respecting surface friction a large amount of power still remains to be accounted for. But it may be said the co-efficient of friction taken for these screws was too small. To go to extremes, let us find the co-efficient of friction on the supposition that the whole of the difference of the power as expended on the four blades of No. 1 screws, and the two blades of No. 2 screws was absorbed in surface friction, and we shall then find the power remaining available for the propulsion of the ship, and to overcome other resistances and see whether this appears to be enough. The question of augment of resistance will be considered farther on.

The trial of No. 2 screws at the 15·726 knots' speed gives a ready means of making the comparison. The power developed to obtain this speed was 4,370 with 88·9 revolutions, and the power which would be required with No. 1 screws to give the same speed of ship would be 6,080, with 85·75 revolutions.

The blade friction of No. 2 screws with the co-efficient of ·0062 was shown to be equivalent to 530 horse-power, and similarly with the same co-efficient No. 1 screws to give the same speed of ship would absorb 897 H.P. The latter power divided by the former gives a ratio of 1·689.

If F be taken to represent the horse-power absorbed in the surface friction of No. 2 screws at 15·726 knots speed of ship, then whatever the co-efficient, may be 1·689 F will represent the horse-power expended on the surface friction of the No. 1 screws at the same speed of ship. If P be taken to represent the horse-power left after the assumed screw blade friction has been deducted, P will be the same for each screw to produce the same speed of ship.

$$\begin{cases} 6080 - P = 1{\cdot}689\ F \\ 4370 - P = \qquad F \end{cases}$$

$$6080 - P = 1{\cdot}689\ (4370 - P)$$

$$\cdot 689\ P = 1{,}300$$

$$P = 1{,}890$$

The relation between the amount of power absorbed by blade friction of No. 1 and No. 2 screws at any speed of ship is represented by the proportion of the intercepted ordinates (aa' : bb') of the respective curves shown in Fig. 14, Plate VIII.

In the case of No. 1 screws 6,080 − 1,890 = 4,190 H.P. would thus be absorbed in friction, and in the case of No. 2 screws 4,370 − 1,890 = 2,480 H.P. This would correspond to a co-efficient of friction = ·0288, or about 4·65 times that the co-efficient taken in the first calculations.

It is evident that this latter estimate of the blade friction is far too great, for, as shown

by the above calculations, it would absorb all the power developed by the engines except 1,890, and this small amount which is left would have to overcome—

1st. The initial or dead load friction of the machinery.
2nd. The working load friction of the machinery and pump resistances.
3rd. The slip of the screws.
4th. The resistance of the ship at 15·726 knots' speed.
5th. The augment of resistance due to the action of the screws.

Now, according to Mr. Froude's experiments with the model of the *Iris*, the I.H.P. necessary to overcome the resistance of the hull alone—*i.e.*, the E.H.P.—would be 2,160 at 15·726 knots' speed. We see, therefore, that blade friction cannot be taken to account for anything like the whole of the bad performance of the No. 1 screws, and we must look for other sources of loss.

1st. It is probable that the four broad-pointed blades of No. 1 screws were more fruitful in eddy making than the others, and encountered more loss from this cause; then, as the blades followed so closely on each other, taking, as it were, slices of water about 4½ feet thick, they might disturb injuriously the water for each other. That is, each blade probably came in contact with eddies set up by the blade which preceded it, particularly towards the points of the blades, where they were broadest and the velocity greatest. The eddies would, no doubt, be worst where the blades approached the sides of the ship, and the heads of the A brackets which carry the screw bearings.

As the blades of No. 2 screws followed each other at intervals about twice as long, any eddies set up by one blade would probably be left behind before the other could sweep round upon them.

It may be said in objection to this view, then why was the performance of No. 3 screws, which had also four blades, so good? These screws were of less diameter and area, and of rather more pitch; the blades had narrow points, and they did not approach so close to the sides of the ship by 1 ft. 1½ in. The blades did not attain the same width as those of No. 1 screws until a distance of 6 ft. 3 in. from the sides of the ship was reached, and such blades would probably suffer much less rotary obstruction from disturbed water.

The second source of loss with the No. 1 screws must be, I think, in the greater augment of resistance with them than with the other screws. The four broad-pointed blades of large area set so nearly square to the line of motion of the ship, following each other so quickly, and approaching so near to the sides of the ship, must interfere, much more than the other screws, with the stream line motions of the water closing in at the stern to balance the head resistance.

Mr. Froude estimates that the disturbance of stream line motions, by the screw in well-formed single screw ships, is equal to from forty to fifty per cent. of increase upon the resistance of the ship, when towed at the same speed. With twin screws there is good reason to believe that the augment of resistance has a less percentage than with single screws, but on

the other hand, there is the drag of the brackets or struts and the shaft tubes to be overcome, and in the *Iris* this drag is known to be exceptionally great. The run of the *Iris* is, however, very fine (as you will see by the section at the screws (Figs. 1, 2 and 3), and the screws may exercise less than the usual influence on the stream line motions, as compared with most twin screw ships. Still the amount of influence exercised must in itself be large, and a consideration of this may help us to understand why the original screws of the *Iris* gave so much worse results than the others.

The great amount of attention which has of late years been given to the improvement of the designs of ships and engines has, it is feared, for a time, prevented such full consideration from being given to the improvement and proper adaptation of the screw propeller as such an important subject deserves. It is understood that cases similar to that of the *Iris* have occurred in the mercantile marine, and if those who have had experience of any such cases would lay the particulars of them before the Institution, they would help to elucidate a subject on which much has yet to be learned.

H.M.'s Twin-Screw Steel Despatch Vessel "Iris."

Principal particulars of Ship and Machinery.

Ship.

Length	300 ft.
Breadth	46 ft. 1 in.
Displacement at trial draught	3,290 tons
Coal capacity	750 tons

Engines.—The two sets of engines are placed in independent engine-rooms, separated by a transverse water-tight bulkhead. Each set consists of four cylinders, two high pressure, 41 in. in diameter, and two low pressure 75 in. diameter. The high-pressure cylinders are bolted to the fronts of the low-pressure cylinders, and are partly recessed into them. The length of stroke is 3 feet. The high pressure cylinders are fitted with expansion valves on the backs of the slide valves.

An arrangement of change valves and pipes is fitted to the cylinders, by means of which each is put into direct communication with the boilers and condensers. The engines are thus capable of being worked as simple engines with steam of low pressure. A surface condenser constructed of brass is fitted in each engine-room at the backs of the cylinders. The tubes are $\frac{5}{8}$ in. in diameter, and the total cooling surface of each is about 7,000 square feet. One vertical air-pump to each set of engines, 43 in. diameter and 21 in. stroke, is driven by a rod off the foremost low-pressure piston of each set, and by bell crank levers.

Two centrifugal pumps, 3 ft. 9 in. in diameter, are employed for circulating the water through the condensers. These are capable of pumping 500 tons of water per hour from the ship in the event of a leak.

The crank shafts are of wrought iron, $15\frac{1}{2}$ inches in diameter. The propeller shafting,

H.M.S. "IRIS."—EXPERIMENTAL TRIALS.

	First Series.						Second Series.				Third Series.					Fourth Series.				
	1	2	3	4	5	6	1	2	3	4	1	2	3	4	5	1	2	3	4	5
Date of Trial	Feb. 4, 1878.				Feb. 5, 1878.		Feb. 15, 1878.				July 3, 1878.					Aug. 1, 1878.				
Draught of Water { Forward / Aft.	15 ,, 7½ 20 ,, 6½						15 ,, 7½ 20 ,, 6½				15 ,, 8 20 ,, 6					15 ,, 8 20 ,, 6				
Immersed Mid Sec. Sq. ft.	700						700				700					700				
Displacement. Tons	3,290						3,290				3,290					3,290				
Revolutions per minute	91·04	82·148	65·105	43·358	24·25	18·0	88·89	81·175	65·07	45·075	97·189	85·388	61·343	40·963	21·6	93·25	76·93	59·385	39·15	19·26
I.H.P.	7[illegible]03	5251	2560	755	217	143	4368	3306	1637	571	7714	5108	1833	606	184	7556	3958	1765	596	163
Speed—Knots	16·577	15·123	12·064	8·187	—	—	15·726	14·517	11·58	7·95	18·573	16·564	12·279	7·797	—	18·587	15·746	12·475	8·321	—
$\frac{(\text{Speed})^3 \times \text{Mid. Sec.}}{\text{I.H.P.}}$	425·0	461·1	480·1	506·6	—	—	623·3	647·8	664·0	616·0	581·4	628·8	707·0	547·5	—	594·9	690·5	770·0	676·7	—
$\frac{(\text{Speed})^3 \times (\text{Dispt.})^{\frac{2}{3}}}{\text{I.H.P.}}$	131·3	145·7	151·7	160·1	—	—	197·0	204·7	209·8	194·6	183·7	196·8	223·4	173·0	—	188·0	218·2	243·3	213·8	—
Screws. Description	Common with 4 parallel blades.						Same screws as before, with two alternate blades removed from each.				Modified Griffiths, 4 blades.					Modified Griffiths, 2 blades.				
Diameter	18 ,, 6½						18 ,, 6½				16 ,, 3½					18 ,, 1½				
Mean Pitch	18 ,, 2						18 ,, 2				19 ,, 11½					21 ,, 3¼				
Slip per cent.	1·57 neg	2·69 neg	3·36 neg	5·33 neg	—	—	1·99	0·93	1·41	2·29	2·97	1·50	1·63 neg	3·36	—	5·04	2·49	0·07	1·25 neg	—
Expanded area of all blades .....sq. ft.	194·4						97·2				144					112				
Disc area of all the blades .....sq. ft.	160·0						80·0				120·0					98·0				
$\frac{\text{Disc area}}{\text{Area of circle swept by tips of blades.}}$	·296						·148				·288					·190				

with the exception of the aftermost lengths, which are of solid wrought iron, are made hollow, of Whitworth's fluid compressed steel.

The diameters of the lengths inside the ship are 14½ in. outside and 8 in. inside; of the lengths in the stern tubes, 16 in. outside and 10 in. inside; of the lengths outside the vessel, 15½ in. outside and 9½ in. inside. The aftermost lengths of wrought iron are 15½ in. diameter. The length of shafting outside the ship is 53 ft. 6 in. to the centres of the propeller bosses. Two brackets 20 ft. 6 in. apart are fitted on each side of the ship to support the shafts, which are covered with thin plate-iron casings to prevent ropes, &c., from fouling them.

Boilers.—There are twelve boilers, placed in two boiler-rooms, which are separated by a transverse water-tight bulkhead. The boilers face each other, and are fired from middle line platforms. The shells of the boilers are made of the same material as the hull of the ship, viz., Landore steel.

The following are the dimensions of the boilers:—In after boiler-room, six three-furnace boilers, 12 ft. 6 in. wide, 13 ft. 6 in. high, 9 ft. 7 in. long. In the forward boiler-room, two three-furnace boilers, 12 ft. 6 in. wide, 13 ft. 3 in. high, 9 ft. 7 in. long; and four two-furnace boilers, 12 ft. 2 in. diameter, 9 ft. 6 in. long. Diameter of the furnaces, 3 ft. 1½ in. Length of fire grates, 7 ft. Total area of grate, 690 sq. ft. Diameter of boiler tubes, 3¼ in. Length of boiler tubes, 6 ft. 5¼ in. Total tube surface, 15,900 sq. ft. Load on safety valves, 65 lbs.

There are two fixed funnels, with ventilating pipes up their centres. The diameter of the forward funnel is 7 ft. 8 in., and of the after funnel 8 ft. 4 in. The height of the funnels above the dead plates of the lower furnaces is 67 ft.

DISCUSSION.

Mr. W. Denny: My Lord, if the opinion which we had last night from a justly renowned shipbuilder with regard to the value of measured mile trials were accepted, I am afraid that this Institution would be obliged to treat this most valuable and excellent paper as a trifling with its feelings, and a contemptuous treatment of its experience and understanding. We would be obliged to ask that all these elaborately delicate and distinctively conducted trials should be replaced by a series of logs of voyages at different speeds and in all kinds of weather. I ask this Institution to consider whether the result would be clearer, whether it would be better, and whether it would be more advantageous for the advancement of the science of naval architecture. Now, passing over all this, and expressing what is my own frank opinion, it is that I am glad this paper has been read before us, because it marks once more that the Admiralty of this country are taking the lead in this question; and no naval architect who has, as I have, a sincere respect and admiration for that great body and its skill, can help feeling glad that they have come forward and produced a paper which, I think, on the special subject of steam trials, has not been equalled in the history of this Institution. I do not think that I over-praise this paper in any way. Looking to the honesty with which these trials have been conducted, and the careful analysis with which Mr. Wright has separated all the results—how he has avoided the temptation of running rashly to the supposition that the friction of the blades alone causes all the difference—how delicately he has divided one point from another—I think we must again congratulate ourselves that such a paper has been read before us, and I think

that this paper stands out as proof, if you will look at those curves, that there is no necessity with progressive trials for either picked stokers or picked coal. Both these items can be safely left over as a superfluity, because both are not of any absolute necessity. What curve of the four that are before you was the one that interpreted the difficulty which surrounded the *Iris's* case? It was the dotted line in black, the curve of the lowest maximum speed, a curve with two blades taken off the original propeller which did not permit the engines to be driven up to their maximum indicated horse-power, and which did not necessitate picked coal or picked stokers. That curve stated the first interpretation of the question, and in the same way properly conducted progressive trials have no need of a high maximum speed for the purpose of interpreting their results. I may state further before you that so far as the trials of my own firm are conducted, they are untarnished in honesty; and I know this, that the trials personally conducted by the majority of the firms on the Clyde are untarnished in honesty, and there is no man who would dare say one word against them. It may astonish gentlemen here to know that one-half of these trials are conducted without the owners or the owners' representatives being there, and we never supply the information unless the owner has a desire for knowledge sufficient to induce him to ask for it. The information is got for our own private purposes. I now turn to make a few remarks on particular items in Mr. Wright's paper. Mr. Wright has spoken about the small initial friction that existed in the *Iris*, and I think he is quite right in giving all credit to the engineers for the small initial friction, because it is very small—8 per cent. He quotes from Mr. Froude's paper, which dealt with the trials of a ship built by my firm, the *Merkara*, in which the initial friction was 13 per cent. of the maximum indicated horse-power. But the trials of the *Merkara* were exceptional from some cause or other in this initial friction. We measure our initial friction by what practically comes to the same thing as Mr. Wright's measurement in tons, by the mean pressure of steam referred to the large cylinder. That mean pressure, as most of the engineers here must be aware, varies from 25 to 28 lbs. Now we have had, and not only we, but many of our friends have had, initial friction which varied from 1·8 to 2·5 pounds—that is to say, from a percentage slightly below 8 per cent. to a percentage of about 10 per cent., supposing you take the minimum mean pressure at about 25 lbs. But there is another point with regard to this initial friction which has given me great pleasure, and that is the way in which these experiments of Mr. Wright's have confirmed the value of the discovery of Mr. Froude, that the initial friction does exist and can be measured. Mr. Wright's experiments with the engines running disconnected have proved that with a reasonable addition for the shafting friction, the initial friction shown by the curves is a reality. I can add to that proof something further. We have been fortunate in the case of three ships, to have them tried, not only at a light draught, but at a load draught. Now the result of these trials was this, that within a very small range the initial friction at the load draught corresponded to the initial friction at the light draught, and corresponded so closely that any little difference might be accounted for. This is an additional proof and confirmation of the reality of the great discovery of that ablest member we have, Mr. Froude. Mr. Wright has very properly said that similar cases to the *Iris* have occurred in the mercantile marine: I am sorry they have not occurred with us. I would have been glad of a blunder to have had such a noble success. But a similar case did occur with the trials of the last large Peninsular and Oriental steamer, the *Kaiser-i-Hind*. I see before me gentlemen who took great interest in her trials, and who worked them out and showed all the energy that one could desire in the matter, and I believe it is quite within the bounds of probability that that Company may be induced to add, in the spirit of which Mr. Ravenhill speaks of its old trials, a new set of trials, those of the *Kaiser-i-Hind*, to the contributions which it has made already to our knowledge. I am certain that every naval architect here would say to that Company that he appreciated the experiments and the analyses

which it had made in that case. Gentlemen, I cannot sit down without again saying how glad I feel that the Admiralty of this country occupy the position that they always ought to occupy, and which I believe they always will occupy, of being in the forefront of naval architecture.

Mr. Henry Wimshurst: My Lord, with your permission I should like to state that I designed and built the first two screw steamships. In the first of these, the *Archimedes*, the screw was made too large for its aperture, and I obtained permission to alter it, or to make another screw. I made another screw, smaller in diameter and in length, but of a greater pitch. When tried, this smaller screw gave an increased speed of two knots per hour. The alteration in the screws of the *Iris* is very similar to that which I made in the *Archimedes*, and affords like results. In the early application of the screw, and in many vessels of modern construction, the apertures are not nearly so large as they should be, nor is the screw far enough from the body of the ship, in consequence of which the propeller blade when passing these parts or places, unduly breaks the water or creates a partial vacuum; the following blade on its approach meets the water falling into such space at great velocity, and in its rear leaves a similar condition of things for the next blade: it is this which causes the jumping motion in the stern of screw vessels, and absorbs a considerable part of their engine power. I have always advised that the screw should be kept as clear of the vessel as is possible, and now I have nothing to add, except that I am gratified to find, that what I did, and what I advised, forty years ago, is beginning to be accepted as sound in practice.

Mr. A. C. Kirk: Mr. chairman, it is with the greatest pleasure that I have listened to this paper. Fortunately for the profession in general, the *Iris*, by a fit of perversity, so to speak, fell at first so far under what was expected of her, that these trials were undertaken. Very fortunately Mr. Denny had pointed out the method of progressive trials for investigating such cases; very fortunately, also, Mr. Froude had investigated the resistance of the hull of the *Iris;* and, also very fortunately, we have Mr. Wright, a gentleman who has made and given us the most complete analysis of these results, and brought out the *Iris* to a triumphant success. Unless I misunderstand the paper, one knot more was got than Mr. Froude's experiments indicated ought to be got. I do not know, of course, under what conditions Mr. Froude's experiments were made. A more full explanation would be very interesting. Most of the paper is so very clearly written, that it is beyond criticism or remark. I would, however, come to the question where Mr. Wright speaks of the engine friction. Now the engine friction is deduced in a method pointed out by Mr. Froude as strictly correct, if the data on which you go are to be thoroughly relied on. Unfortunately, the low part of a progressive trial curve is generally the least absolutely correct. In my own case, in analysing a good many engines, the variation of engine friction shows that while it may be actually the case that the engines do show this actual variation in friction, I must say that the data at that part of the curve are often a little doubtful, and I am not quite prepared to accept a statement that the engines do show quite such a large variation in friction as the curves would indicate. No doubt the largest element of friction is the state of the piston packing. The friction of the bearings, of course, will tell; but I do not attach so much importance to that, because if the bearings of the engine are so tight as to materially increase the friction, the heating of those bearings will very soon draw attention to the act, and it will very soon have to be remedied. Therefore, until excessive friction is clearly proved to exist in an engine, I do not think it is *primâ facie* at all probable that the state of the bearings has very much to do with it; but the piston packings admit of being put very tight—obnoxiously tight, and as the steam carries off the heat of friction of the piston, we are never the wiser until sometimes the mischief is done and the cylinder is cut. I once did know an engine where the piston packing was so tightly set up by a very stupid man, who wished to make it very perfect and steam-tight, that in less than half an hour the packing ring had so thoroughly cut and fired into the cylinder, that the

pressure of the steam, aided by the other cylinder, pulled the centre out of the piston, and left it in bits at the bottom of the cylinder. That is an actual case that occurred, but only once in my knowledge, therefore I give it merely as an illustration. I find, besides deducting the engine friction from the curve, Mr. Wright has tried some very valuable experiments with regard to running the engine disconnected. The horse-power got there was 398. Of course the friction of the engine under these conditions was 100 per cent. of the whole power. To some extent friction is increased when the load on the piston was increased, but to what extent I do not know exactly. The friction of the bearing, no doubt, would increase very nearly as the mean pressure of the piston. The friction, however, of the packing rings, which probably is the largest part of it, would not necessarily increase at that rate. Perhaps it might not increase at all; probably, however, to some extent it would increase, because there is generally an atmosphere of steam behind the packing rings, as is evidenced by the amount of oil and dirt that gets into them. That is one point on which I feel a little inclined to dissent from Mr. Wright's paper in assuming that 398 was the horse-power of the friction under a full load; I fancied it was more. I think a very valuable set of deductions, indeed, and a very original one, are those in which Mr. Wright analyses the waste of the power in the screw propeller, showing very clearly, in fact, that blade friction will by no means account for anything like the power that was wasted there. The screw propeller works in a very confused current, and possibly the larger diameter of screw loses a good deal of power, because at the one side next the vessel it moves in water, and sets in motion water, that is more or less moving along with the ship, while at the tips of the blades probably these large screws were passing through water that was flowing past the ship. Now as the slip was almost nothing, the probability is that the outside edge of the blades here were moving edgewise through the water for a short space, while those on the inside were violently and suddenly moving a lump of water, in both of which operations there is evidently a waste of power, and the smaller screw evaded that difficulty simply by keeping out of the way of these two extreme cases. I happened to see a case that convinced me very strongly that there are those confused currents. In the case of the *Cyclops*, twin screw ship, the engines were fitted with the usual disconnecting arrangement, whereby the pins in the coupling can be drawn back with screws. In order that that might be done easily, the pins were a little slack; they were rather too much slack, in fact. So long as we were running straight on at full speed, all went quietly and nicely, but as soon as that was over, and we began to do circles and put the rudder hard over, it was a great puzzle to know what was going on, and first one engine, and then in a short time the other, made a noise uncommonly like riveting going on in the engine-room. Presently I could trace it to this, that the screw that was going sideways into still water—that is to say, if the stern was going to starboard—as the ship went round the starboard screw set up this hammering, and it was just the reverse if the stern went to port. The engines were making, if I recollect rightly, a very small amount of negative slip—not very much. I take it the action was this: as the screw blades got into the outside water flowing past the ship, a portion of each blade got a blow in the back, which caused this back wash. I fancy that was the explanation of it. At all events it came to me as a sort of ocular demonstration that there was a very confused current in which the twin screws were working. I have nothing further to say, except to repeat, what I scarcely need repeat, the high appreciation we have of such a paper as has been given to this Institution, and it will be a lasting memorial of the progress our Admiralty friends have made, and of what they have contributed to this Institution and to the scientific world.

Mr. John R. Ravenhill: This paper has followed one read last year, and discussed, in which the subject of twin screw propulsion formed a very prominent feature, and I am sure we must all feel deeply indebted to Mr. Wright for the great trouble he has taken in laying the several details

before us, and adding so very much to the knowledge of various members of this Institution. There can be no doubt that the fact of the *Iris* being unsuccessful in the first instance, to a certain extent, led to a series of trials which, had it not been for that inopportune result, would probably not have been carried out in the way in which they have been. I was myself connected for a long series of years with the Admiralty officials in very, very many trials which were made. I have always spoken of the great care taken in conducting them, and I cannot refrain from availing myself of this opportunity of personally expressing that very valuable information is due, and in fact has always been due, to the results obtained by the measured mile trials conducted under Admiralty supervision. As an old friend of Mr. Wright's, I congratulate him on the great success of his paper.

Admiral Ryder: My lord, I should like, with the permission of the meeting, to make a very few remarks on a part of this subject which has been very much talked about lately. I have the advantage of knowing Mr. Froude intimately, and I dare say it is known to all of you that he is in a state of very delicate health, and went abroad some months ago, and I believe he is still absent. He went to the Cape. Previous to that I visited him and saw some of his experiments, and he was then engaged on the question of the dimensions of these screw propellers. The question of the friction of the surface of the screw is one which is of very great practical importance to us at sea in our ships, because we have the surface of our screws, by the action or formation of shells upon them, turned from the very smooth surface they have when they leave Mr. Wright's supervision, into particularly rough surfaces, and we find that that roughness detracts from our speed in a most remarkable degree. It is of great importance, and I think you will allow me to make this practical deduction from it—to keep our screw surfaces polished as much as possible. As naval officers we get copies of your discussions, and it may be advantageous to the younger ones to know that the first thing they should do on coming into harbour, if they cannot raise their screw, is to send down a diver to clean the surface of it if it requires cleaning. It takes an enormous amount of our speed out of us if we have a very rough screw. I may, perhaps, speaking of the question of surface friction, allude to what Mr. Froude also drew my especial attention to, that whereas we like the whole of the bottom of the ship smooth, it is much more important that the bows of the ship should be smooth than the part abaft the midship section. If naval officers did not know how long they were to be in harbour, their first business would be to polish their screws, then their bows, and after that they might leave the rest of the ship, not paying any particular attention to it.

Captain Colomb: My lord, I should like to add one word to what Admiral Ryder has said. He has spoken of the probability of doing these things—of cleaning the surface of the screw so as to get rid of the surface friction. Now, in the ship that the gallant Admiral's flag flew in, in China, we suffered very much from the incrustation of the screw, and also of the surface of the bow of the ship, and it became after a little time our constant habit to do as the gallant Admiral has suggested—that is to say, to scrape and polish by means of divers the bows of the ship if we had time, but certainly the surface of the screw itself, and we always found that we gained very considerably in economy of coal by that process. It was not a long job to clean and scrape the surface of the screw, but we always found that we got a good result in economy of coal afterwards.

The President: I am sure I shall only speak the general sense of the meeting if, before I ask Mr. Wright to reply upon the observations we have heard, I tender to him our thanks for a paper of great ability and very great interest.

Mr. J. Wright: My lord, with your permission I should like to thank the gentlemen who have noticed the paper in the complimentary terms in which they have done. I should like especially to thank Mr. Denny, who has done so much in the same line himself. Any expression of approval from Mr. Denny I can only say I highly appreciate. With reference to the question of measured mile

trials; generally these trials were made with picked coal and picked stokers. In the case of the *Iris* the picked coal was not necessary, but the picked stokers were necessary, and for this reason: it was desired to keep the steam uniformly at the same pressure at each of the different speeds, and this is better done with picked stokers than ordinary ones. We stationed a trained man at the steam gauge, in the stoke hold, so that the least variation, even of a pound, was seen, and he directed the stokers to manage the dampers, so as to keep the steam uniformly at the same pressure. With regard to the question of jockeying at measured mile trials, I think those who have attended the Admiralty trials must know that that is obsolete. With regard to jockeying the feed, everyone knows that with surface condensers, such as we have, if they tried it, they would very soon come to grief. With reference to the engine friction, I can only say that I agree with Mr. Denny and Mr. Kirk, that the greater proportion of the variation is in the piston friction, and probably also a little in the slide valves, and in the stuffing boxes. Of course, when the main bearings are well adjusted, any unusual friction will very soon show itself. I am pleased to find that the *Iris* worked with so little friction. Mr. Kirk has asked why we got more speed with the *Iris* than we expected. At the time the experiments were made, we did not very well know what percentage of the gross horse-power the effective horse-power would be. It turned out that she has a larger effective horse-power compared to gross horse-power than was expected, therefore we got a better result. With reference to Admiral Ryder's remarks about the surface friction of the screws, the experiment of polishing the blades of No. 3 and 4 screws did not prove to be of what I should call permanent value; because when the blades of No. 3 were removed, after having been in the water for about ten days, the surface appeared as if it had been covered with finely-powdered lime hardened, and when you put your hand over it, it felt as if you were rubbing your hand on fine sand-paper. Then with regard to the blades of No. 4 screw, they had only been in the water about three or four days, when they began to exhibit the same appearance and to give the same rough feeling to the touch. I think the best thing to do is to get the surface tolerably smooth to commence with, to clean the screws periodically, and when the vessel is docked, coat them with a good composition.

THE STRUCTURAL ARRANGEMENTS AND PROPORTIONS OF H.M.S. "IRIS."

By William H. White, Esq., Assistant Constructor of the Navy, Member of Council.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879; the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

The use of mild steel as a substitute for iron in shipbuilding is now attracting so much attention, that some interest will probably be felt in the brief description of the structural arrangements of H.M.S. *Iris* which is given in this paper. It is well known that this vessel was the first, built in England, in which mild steel was employed; and that she was the first vessel of the Royal Navy wholly built of steel. Notwithstanding her exceptional character, as compared with other types of war ships or with merchant ships, it may be well to put on record in the Transactions, the facts as to her scantling and structural strength; for while she differs from most seagoing ships in many particulars, she illustrates, I think, some principles of construction that are applicable to all steel-built vessels.

It is necessary at the outset to sketch the general features of the design. The principal dimensions are as follows: Length between perpendiculars, 300 ft. Breadth, extreme, 46 ft. Depth, from upper deck, at side amidships, to under side of bar keel, 28 ft. 4½ in. Depth of keel, 1 foot. Mean load draught, 19 ft. 9 in. Corresponding displacement, 3,735 tons.

Mr. Wright has given in his paper a full account of the propelling machinery, and the remarkable steam trials of the ship. I need not repeat the description here.

Fig. 1, Plate IX., shows in outline the manner in which the hold-space is occupied and subdivided. No less than 138 ft. of the length is occupied by engines, boilers, and coals; there are two separate stokeholds and two engine-rooms. The engines and boilers are kept below the water-line, and protected throughout by wing coal-bunkers, varying in thickness from 5 ft. to 8 ft. between the upper and lower decks, and decreasing in thickness as the depth below water increases. These bunkers are more clearly shown in the "Midship section" (Fig. 3, Plate X.) The outline "Profile view" (Fig. 2, Plate IX.) shows the heights of the decks and platforms, the positions of the bulkheads, &c. Reference to these drawings will show that there are *ten* complete watertight transverse bulkheads; and that in wake of the stokeholds, where the main bulkheads are 44 ft. apart, intermediate or "partial" bulkheads are introduced in the coal-bunkers. The longitudinal bunker bulkheads are also watertight; and throughout the length occupied by the machinery and boilers there is a watertight inner skin, forming a complete

double bottom (see Fig. 3, Plate X.) across the ship between the bunkers. Before and abaft the double bottom, a watertight platform is built about 7 ft. below the lower deck, and extended to the bow and stern respectively. This platform does not extend quite to the foremost boiler-room bulkhead, because the small "fore-hold" (about 14 ft. in length) is nearly occupied with water-tanks and chain-lockers. In the coal-bunkers also there is a platform about mid-height between lower deck and bilge (see Fig. 2, Plate IX.) As is usual in Her Majesty's service, the magazines and other enclosed spaces are formed into watertight compartments; and altogether, there are no less than 61 separate compartments; 21 in the hold proper, and 40 in the double bottom and bunkers. The various partition bulkheads just mentioned of course contribute largely to the structural strength, especially to the transverse strength; and this must be kept in view when examining the framing. In some respects the system of framing adopted in the *Iris* resembles that commonly used in iron vessels of the Royal Navy, but in other respects it is novel. It may be said that the arrangement of the framing has been governed by the desire to efficiently stiffen and support the comparatively thin skin-plating, while avoiding the use of closely spaced transverse frames. The skin-plating is $\frac{1}{2}$ in. thick, with a doubled sheer-strake; the garboard strakes are $\frac{5}{8}$ in. Throughout the length of the double bottom the transverse frames are spaced 4 ft. apart; before and abaft the double bottom the spacing is $3\frac{1}{2}$ ft. By means of the longitudinal framing, shown on Figs. 2 and 4, the unsupported spaces of the bottom-plating are kept down to 16 or 20 sq. ft., and buckling or unfairness is prevented.

The midship section (Fig. 3, Plate X.) represents the system of framing in wake of the double bottom. The outer or frame angles are $6 \times 3 \times \frac{7}{16}$; they are made continuous from the first longitudinal on one side to that on the other, passing through scores cut in the centre or vertical keel-plate, and are again continuous from the upper deck down to the second longitudinal. Up to the turn of the bilge the transverse frames below the inner skin are on the well-known "bracket-plate" system, the brackets being formed of $\frac{5}{16}$ in. plates, and the short angles on their inner edges being $6 \times 3 \times \frac{3}{8}$ between the keel and second longitudinal, but only $3 \times 3 \times \frac{7}{16}$ between the second and third longitudinal, where a lightened plate is substituted for a bracket. Within the coal-bunkers the reversed frames are $3 \times 3 \times \frac{7}{16}$. The centre-plate keel is $\frac{3}{8}$ in. thick, with lightening holes in the spaces between the frames; its lower edge is riveted to the bar keel (iron). The longitudinals in the double bottom are formed of $\frac{5}{16}$ in. plates, with $3 \times 3 \times \frac{3}{8}$ bars on the outer edges and $2\frac{1}{2} \times 2\frac{1}{2} \times \frac{5}{16}$ bars on the inner edges, as shown in Fig. 5, Plate XI. Plates and bars are worked continuously, with butts carefully shifted and strapped, in the same way as is usually done in armoured ships. The first longitudinal out from the keel on each side is made watertight throughout the double bottom. In wake of the bunkers the light transverse framing is well stiffened by the lower deck stringer, and by the platform below the lower deck; besides which there are longitudinals formed of $10 \times 3\frac{1}{2} \times 3\frac{1}{2} \times \frac{7}{16}$ Z-bars (similar to those now used for frames behind armour in ironclads). These Z-bars are scored in over the 6 in. frames and secured to the skin-plating; a continuous $3 \times 3 \times \frac{3}{8}$ angle-bar being worked inside the reversed frames and riveted to them and to the Z-bars. A strong and simple stiffener is thus secured with very little workmanship.

Outside the limits of the double bottom the framing is of the character shown in Fig. 4, Plate X. It closely resembles that described for the coal-bunker spaces, with the addition of a floor-plate, and a gutter-plate at the middle line. Care is taken to scarph-on the Z-bar longitudinals before and abaft the double bottom to the deep longitudinals within the double bottom. Fig. 6, Plate XI. shows an outline of the arrangement. The plates of the longitudinals are gradually reduced in depth, and a strake of the inner bottom is extended over the tapered portion of the longitudinal to strengthen the connection.

The inner bottom-plating is $\frac{5}{16}$ in. and $\frac{1}{4}$ in. in thickness; the edge-joints are single-riveted, and butts double-riveted. It is worked flush, as is usual in ships of the Navy. Its vertical continuations, forming the coal-bunker bulkheads, are of $\frac{1}{4}$ in. plating with $3 \times 3 \times \frac{3}{8}$ vertical stiffeners placed 30 in. apart. This plating is lapped and single-riveted at the edges, the butts being double riveted. The coal-bunker bulkheads are continuous, the transverse bulkheads being cut off where the two systems cross one another. This is a departure from common practice, but has obvious advantages. For nearly one-half of the length of the ship, where all the principal strains have to be resisted, and where the largest hatchways have to be cut in the decks, the ship's sides are constructed strictly on the cellular system, the inner and outer skins being stiffened independently as well as strongly connected to each other by the horizontal plating on deck and platforms, and by the vertical transverse bulkheads. The desire to provide coal-protection for the engines and boilers was the primary cause of this arrangement; but it might probably be imitated with advantage in other classes of ships.

The outer bottom plating up to some distance above the bilge is worked in the usual manner with double chain-riveted lap joints. Above this height the plates are flush-jointed at the edges, with continuous single-riveted internal strips; this is purely for the sake of appearance. The butts are all secured by double chain-riveted straps. It may be proper to state here that Staffordshire iron rivets are used throughout the ship, the sizes being the same as would be used with iron plates of the same thicknesses, but the pitch being reduced slightly in the butt fastenings to secure sufficient shearing strength. This was preferred either to using larger iron rivets, and consequently having heavier straps and laps, or to using steel rivets.

Respecting the transverse and other bulkheads in the hold, it will suffice to say that the usual practice is followed in construction; the plating is $\frac{1}{4}$ in. and $\frac{5}{16}$ in. thick, with vertical stiffeners $3 \times 3 \times \frac{3}{8}$ placed 30 in. apart. Single riveted lap joints are used in this work.

Considerations of durability are really paramount in the determination of the minimum thickness of plating that can be used in most of these partitions, and the gain by substituting steel for iron is comparatively unimportant.

There is nothing in the deck-framing which needs to be described; but it may be interesting to summarise the arrangements of the deck plating:—

Upper Deck.—A partial steel deck ($\frac{3}{8}$ inch) on each side, about 8 ft. wide amidships, forming a top to the coal bunker; before and abaft the bunkers this plating is gradually tapered to a stringer about 4 ft. broad.

Lower Deck.—A stringer plate, 30 in. wide, and $\frac{3}{8}$ in. thick.

Platforms.—Before and abaft double bottom (watertight) $\frac{1}{4}$ in. plating. In coal bunker $\frac{5}{16}$ in. plating.

The butts of all the longitudinal tie-plating are carefully strapped, and double chain riveted.

The framing of the extremities calls for very few remarks. A bar-stem (iron) is used, and the bow-framing is of the ordinary character. At the stern the only unusual features are consequent upon the extreme fineness of form. In order to decrease the labour of building and the volume of internal spaces requiring to be filled in with cement, the bar-keel is ended about 30 ft. before the sternpost. The true keel from that point is really a flat plate turned upwards, and carried along about 6 ft. above the height of the under side of bar keel produced. The space between the flat keel and the base line is made good by means of an external centre-plate, having double angle irons on the upper and lower edges, and wood chocks bolted to the sides, with frequent bracket-stiffeners in steel. Fig. 7, Plate XI., illustrates these details.

In the *Iris*, owing to her great power and fine form, the length of external shaft tubes is about 50 ft. Each tube is supported by two struts or A-frames; and the attachment of these to the hull has been satisfactorily arranged with only trifling additions to the framing proper. Fig. 7 illustrates this feature. Under the very severe and repeated tests consequent upon the long series of steam trials there has not been the least indication of weakness or want of rigidity at the after part, or indeed at any part of the ship. Another illustration of the possibility of meeting great local strains by comparatively small, but judiciously applied additions to the ordinary framing, is supplied by the engine-bearers of the *Iris*. Fig. 8 shows their general arrangement, and any detailed description is unnecessary.

It is now about two years since the *Iris* was launched at Pembroke. She has not yet been tried at sea, except on the short passage from Pembroke to Portsmouth, but in trials under way, repeated dockings, and under various conditions to which a ship is subjected while incomplete in a dockyard, she has shown herself perfectly successful in resisting local strains. As to her capacity for resisting the principal strains to which a ship at sea is subjected, there can be no question. Her transverse strength is, as has been shown, exceptionally great. As to her longitudinal strength the following facts may be stated. When floating in still water she is subjected to hogging moments throughout her length. Fig. 20, Plate IX., shows the curves of weight, buoyancy, loads, shearing forces, and bending moments constructed in the usual manner. The maximum hogging moment in still water is 19,000 foot tons, or about equal to 1-58th of the product of the displacement by the length. To this bending moment there corresponds a maximum tensile strain on the upper deck of $2\frac{1}{2}$ tons per square inch. Further, if the *Iris* is supposed to be balanced instantaneously on the crest of a wave of her own length (300 ft.) and 15 ft. high, her conditions of strain become modified, as shown in Fig. 20. The maximum hogging moment is then 38,000 foot tons (1-29th of the product of displacement into length). To this bending moment there corresponds a maximum tensile strain on the upper deck of about five tons per square inch, or less than *one-fifth* of the ultimate strength of the material. This is an eminently satisfactory result. The neutral axis for hogging is about

52-100ths of the depth of the ship amidships below the upper deck at the side, so that the compressive strains on the bottom do not exceed a maximum of 4·6 tons per square inch. It is interesting to notice in passing that the maximum hogging moments of the *Iris* in still water and on the wave crest bear to one another a ratio differing from that of the corresponding moments for any ship subjected to hogging strains only in still water, of which the curves of weight, buoyancy, &c., have yet been published. Comparing her with the *Minotaur*, for example :—

		Maximum hogging moment,		
In still water	*Minotaur*	Displacement ×	Length	÷ 88
	Iris	,,	,,	÷ 58
On wave crest	*Minotaur*....	,,	,,	÷ 28
	Iris	,,	,,	÷ 29

It is not necessary to discuss the differences in the distribution of weight and buoyancy, and in the forms of the ships which produce these results, as the corresponding curves for the *Minotaur* are before the public.

The *Iris* is now practically complete, and her weight of hull has been ascertained. It amounts to about 38½ per cent. of the load displacement, all the fittings being included. On investigation it is found that the saving effected by using steel instead of iron may be said to be about 12 per cent. on the weight of hull, or about 175 tons. This saving renders possible a large proportionate addition to the coal supply. The distribution of the weights in this vessel is as follows when expressed as percentages of the displacement :—

	Per cent.
Hull	38·5
Rigging, armament, stores, and general equipment	13·5
Engines, Boilers, &c.	28·
Coals	20·
	100·

The percentage for weight of hull will probably seem high to gentlemen conversant only with merchant ship construction ; and it may be well to remark that in a war ship, carrying guns and torpedoes, minutely divided into watertight compartments, and elaborately fitted throughout with reference to her special service, a large amount of weight is charged to the hull, to which there is nothing corresponding in the merchant ship. The fairest method of estimating the saving of weight effected by using steel is, therefore, that I have adopted—comparing the *Iris* with another iron-built war ship of nearly the same dimensions and displacement. But it may be interesting to add that in looking through the details of weights for the *Iris* I find that over 200 tons of materials have been worked into the hull for fittings, subdivisions, &c., not required in a merchant ship, and if this weight be deducted, the hull absorbs only about 32 per cent. of the displacement, and it could be built, no doubt, with that weight of sufficient strength.

As a matter of interest only, illustrating the vastly different conditions under which a torpedo boat is driven at a speed equal to that of the *Iris*, the following table is given :—

Hull	38	per cent. of the displacement.
Equipment and coals	22	
Engines, boilers, &c.	40	
	100	

To drive the *Iris* at her full speed of 18·6 knots on the measured mile about 2·3 I.H.P. per ton of displacement were required. In the torpedo vessel over 14 I.H.P. per ton are expended. The *Iris* is a sea-going ship, fully equipped, and capable of steaming nearly 7,000 knots at a speed of 10 knots on her own coal supply; but the torpedo vessel is in no sense sea-going, and carries only a limited coal supply and equipment.

The *Iris* gains upon the torpedo vessel in these various particulars, chiefly because of her much greater size, but when she is compared with other fast sea-going steamships her size is very moderate, and her speed is much in excess, in consequence of which the expenditure of power in proportion to displacement is unusually great. This statement is not likely to be questioned by anyone familiar with steam-ship design. The length of the *Iris* is also very moderate when considered in relation to her maximum speed; but it is necessary to remember that she has absolutely no *straight of breadth* amidships, and consequently has as great a length of entrance and run as many merchant steamers 80 ft. or 100 ft. longer than herself. This is, no doubt, much in her favour when steaming at speeds approaching her maximum.

No sea-going merchant steamer having realised the measured-mile speed of the *Iris*, I cannot illustrate the matter by comparing their performances with hers. But turning to Mr. Froude's paper "On the Comparative Resistances of Long Ships" in the Transactions for 1876, and having regard to the experiments on the model of the *Merkara* (built by the Messrs. Denny) a comparison may be made between her probable performance if driven at 18 knots by twin-screws and that of the *Iris*—18 knots being about the full speed which the *Iris* may be expected to attain when fully laden. For a speed of 18 knots, Mr. Froude gives :—

Resistance of *Merkara* (about)	=	70,000 lbs.
Corresponding effective horse-power	=	3,870 H.P.

This is for the naked hull: if she had twin-screws, the external struts and tubes would cause an increased resistance, which would, however, be more than counterbalanced by the greater efficiency of the twin-screws. Using our data for the *Iris* in order to pass from the nett effective horse-power of the *Merkara* to her probable indicated horse-power for 18 knots, I find that the ships compare about as follows, when the *Merkara* is brought to the actual load displacement of the *Iris* :—

Merkara (probable)	8,500 I.H.P.
Iris (actual)	7,500 ,,

At the measured mile the *Merkara* attained 13 knots with 2,000 I.H.P. when driven by a single screw; and the *Iris*, although of 690 tons less displacement at her trial draught (3,980 tons as against 3,290 tons), required about the same power for an equal speed. The *Merkara* has a wetted surface, which is only about *six-sevenths* the wetted surface of the *Iris*, although her displacement is *one-fifth* greater, her form being well adapted for the moderate

speeds at which she was intended to work; for which speeds the frictional resistance, due to the wetted surface, exceeds *three-fourths* the total resistance, according to Mr. Froude's estimate. It is of course necessary to remember that the engines of the *Iris* develop less than *one-third* of their full power at 13 knots, while the *Merkara's* engines are at full power; but, allowing for these differences, the figures now given illustrate the influence which the length of entrance and run have upon the expenditure of power at high speeds, and show that absolute length, if obtained by middle-body, with only moderate lengths of entrance and run, may not be a source of economy at very high speeds. Mr. Froude has fully stated the case in his paper published in the Transactions for 1877. I have referred to it only as bearing upon the dimensions chosen for the *Iris*.

It may be interesting to add a brief comparison of the measured mile performances of the *Iris* and the *Hecla* torpedo depôt and store-ship, of which Mr. Barnaby gave a description in his paper on "Armour for Ships," read yesterday. The *Hecla* is 392 feet long, 39 feet broad, and on her trials had a displacement of 5,760 tons. She attained a full speed of twelve knots with 1,760 H.P., and a speed of 8¼ knots with 645 H.P. For equal speeds the *Iris*, of 3,290 tons displacement, requires 1,670 and 670 H.P. respectively. The apparent gain for the *Hecla* is very considerable, and it is necessary to note the fact that she, like the *Merkara*, being designed for working at moderate speeds, is admirably adapted for the purpose, having a small ratio of wetted surface to displacement as compared with the *Iris*, which is designed for extraordinarily high speed. The case stands as follows:—

	Sq. ft. of wetted surface.
Hecla	4·2 per ton of displacement.
Iris	6·7 ,, ,,

The expenditure of engine power for the twelve knots' speed may be expressed by the following ratios:—

	I.H.P. / Displacement.	I.H.P. / (Displacement)$^{\frac{2}{3}}$	I.H.P. / Wetted Surface.
Hecla	·305	5·5	·073
Iris	·507	7·5	·076

Of these three ratios the first will probably be considered most important in a commercial sense, but the third should not be omitted from consideration, because the adoption of an extremely fine form, adapted for very high speeds, involves the high ratio of wetted surface to displacement which has been stated for the *Iris*. The foregoing comparison requires to be supplemented by other considerations. First, the engines of the *Hecla* were developing their *full power* for the speed of twelve knots, whereas the engines of the *Iris* were developing less than *one-fourth* of their full power. Secondly, the screws of the *Iris* are adapted to the full power of her engines; and for comparatively slow speeds, such as twelve knots, this involves a greater expenditure of power in overcoming screw-friction than is required in the *Hecla* to overcome the frictional resistance of her screw. Experimental data for the *Iris* enable it to be stated that at twelve knots the waste of power on screw-friction, and on the constant friction of the very powerful engines, amounts to 500 H.P. at least, out of 1,670 H.P. indicated. No corresponding data are available for the *Hecla*, but at the very

outside the corresponding waste of power in that ship cannot be supposed to exceed 300 H.P. out of the 1,760 H.P. indicated, and the waste would probably be less. Making these deductions, we have 1,460 H.P. for the *Hecla* and 1,170 H.P. for the *Iris*, available to overcome the other factors in the gross resistance, such as the nett resistance of the hull, augment due to the action of the screws, load-friction of the engines, &c. The expenditure of this power (remaining after the waste work on screw-friction and constant friction of the engines has been allowed for) stands as follows for the two ships :—

	H.P. Displacement.	H.P. (Displacement).$^{\frac{2}{3}}$	H.P. Wetted Surface.
Hecla	·25	4·5	·06
Iris	·35	5·3	·054

These figures are suggestive. They illustrate the penalty which a ship designed for very high speeds has to pay when steaming at moderate speeds, as compared with a ship designed expressly for such moderate speeds. The latter gains in having less powerful engines, less waste work on engine friction and screw resistances, and less wetted surface in proportion to displacement. But, on the other hand, if the forms which are so well adapted for moderate speeds, are carried out in vessels intended to be driven at very high speeds, they may prove very wasteful of power as compared with finer forms, like that of the *Iris*.

In determining upon the dimensions and proportions of the *Iris* it was necessary to consider not merely possible economy of engine-power, but her special requirements as an unarmoured war-ship. The chief requirements additional to those of speed and coal-endurance were :—

(1) Protection of the engines and boilers by placing them under water, and within the side coal-bunkers.

(2) Provision of sufficient initial stability to enable the vessel to be safely navigated when her consumable stores were exhausted, or when compartments had been bilged in action.

(3) Moderate length, to secure handiness.

These special requirements, and her unusually high speed, completely separate the design of the *Iris* from that of the swiftest ocean-going merchant steamers, and render it practically useless to endeavour to argue from one class of ship to the other. No one at the Admiralty would be disposed to recommend that the forms and proportions of the *Iris* should be imitated on a larger scale in the merchant service. Nor, on the other hand, does it seem probable that a swift despatch vessel fulfilling the above requirements could be built satisfactorily on the model of the existing merchant steamers, having, say, ten beams in the length. These proportions are, no doubt, well adapted for merchant steamers carrying heavy cargoes, with centres of gravity low down in the ships, and freely using water or other ballast to preserve their stability. But in a vessel like the *Iris* the vertical distribution of the weights is very different; and if an armed despatch vessel were built on the merchant steamer model, of the same displacement as the *Iris*, but with, say, ten beams in the length, I am of opinion that sufficient initial stability could only be secured, even in the load condition, by using

ballast. This is the conclusion I have reached after a careful consideration of the particulars of many successful ocean steamers, with which I have been favoured by various gentlemen. Furthermore, to the best of my judgment it seems certain that even if any economy of steam-power could be realised in the longer and narrower despatch vessel as compared with the *Iris*, the consequent savings in weight of machinery and coals, would be much more than counter-balanced by the weight required for ballast, and the additional weight put into the hull, if this vessel were to be made as strong and as stable as the actual *Iris*.

It may be said that the *Iris* is unnecessarily strong against principal strains, or that she is unnecessarily "stiff" against heeling forces. In reply, I need only say that with a few exceptions—such as the upper-deck plating — the provision against local strains and for minute watertight subdivision, as well as for a reasonable amount of wear and tear, has regulated the scantlings chiefly, and caused the strength against principal strains to be so ample. Moreover, taking into account the differences between merchant-ship and war-ship construction mentioned above, and the increased strains incidental to the adoption of greater lengths and greater ratios of length to breadth, I am inclined to believe that it would be no easy matter to build a new and longer *Iris* with a less weight of hull than that of the existing ship, and with sufficient strength. As regards subdivision and "stiffness," it is clear that in a ship intended to be capable of fighting, there must be a margin against loss of stability through compartments being damaged by gun fire or ramming, which may reasonably be dispensed with in a merchant steamer.

A minor difficulty that would have to be faced in a narrower and longer ship of equal displacement with the *Iris* would be the stowage of the hold. This would require to be entirely rearranged as compared with the *Iris*, if the engines and boilers are to be kept under water. And accepting the possibility of doing this by occupying a greater length with the machinery and boilers, it will be evident that coal protection equal to that in the *Iris* could only be given to the longer ship by carrying a greater weight of coal. The greater length of side to be protected necessitates this, or the acceptance of a less thickness of bunker if the same weight of coal is carried as in the *Iris*.

In making these remarks on the dimensions and proportions of the *Iris*, I would not be supposed to assert that the ship is incapable of improvement in these respects; my sole object is to indicate the special character of the service for which she was designed, and the manner in which other requirements have been met concurrently with the attainment of high speed under steam. If still higher speeds were desired in future vessels, or if a larger coal supply were required, it is likely that advantage would result from an increase both in length and in the proportion of length to breadth. Greater length is, of course, advantageous in decreasing pitching and enabling speed to be maintained in a sea-way. On the other hand, it must be accompanied by some decrease in handiness, although this might have to be accepted in order to secure higher speed and greater coal endurance. But I am disposed to think that all the requirements in armed despatch vessels for some time to come may be met without approaching the lengths, or ratios of length to breadth, now adopted in many of the largest sea-going steamers of the mercantile marine.

DISCUSSION.

Mr. Thornycroft: My Lord, I would beg to make a few remarks upon this paper, particularly as I feel interested in some of the statements. The proportion of weight of hull and several other particulars given correspond so very nearly with those of boats of my firm, and the description given of the enduring power of the coal supply, that I should like to make a few observations to account for what I do not imagine is meant to depreciate that endurance, but still it would lead one to believe that we can only carry sufficient coals for a run of a few miles. Therefore I beg to make this statement, that in sending boats from the Thames to Portsmouth, and from Chiswick to Cherbourg, we find not the least difficulty in carrying twice the amount of coal required to make the voyage, and in running from the Thames to Portsmouth the coal consumption is about 2½ tons, the coal supplied being 5 tons; and the speed maintained is from 10 to 12 knots. Several French boats in going to Cherbourg encountered heavy weather. Of course heavy weather in the Channel for so small a vessel meant a great waste of coal, but in those cases I think a run of from 200 to 400 knots could be made at a moderate speed with these vessels. With regard to Mr. White's paper, it seems to me to show that the greatest care has been bestowed on the design of the hull. And the wide proportions used giving such good results as to speed seems (I am very glad to believe) to show that Mr. Froude's theory as to the width of the ship, a wide ship with a small surface, gives better results than a long ship with a larger surface. The comparison of the *Iris* and the *Hecla*, in which the fine form of the *Iris* is somewhat handicapped by the large friction of the heavy machinery when worked at a low speed still giving a good comparison, is very interesting, and I am sure that in this paper giving the details of the structure, and Mr. Wright's admirable paper on the performance of the screw, we have such excellent information with regard to this ship, that if we could have through the mercantile navy similar complete information as to what was really built, it would be a very great advantage to everybody, because ships would thus be able to be built with great improvements. Builders who were formerly, no doubt, jealous of other firms knowing exactly what they did, might be influenced by it, and everybody would gain, by the sound knowledge of what constitutes a good ship, being general.

Mr. William Denny: My Lord, would you permit me to say a few words, as Mr. White has referred to the *Merkara* tried by my firm? I quite agree with all the remarks which Mr. White has made as to the difference between these vessels, and which he has made very justly. The *Merkara* was a vessel with 100 ft. of straight amidships, and although her design was suitable enough for the ordinary speed at which she was driven, eleven knots, it was decidedly unsuitable for her being driven faster, because her forebody and afterbody were too short. Mr. Scott Russell taught us that theory long ago, and at the present moment many of our speed calculations are based on measuring the length of the forebody and the afterbody. I must acknowledge to Mr. Scott Russell with real pleasure that we have come round to that. Mr. Kirk has invented an ingenious method by which this theory can be roughly applied. I am much astonished at the results obtained in the *Iris* construction. The thinness of the material is something beyond what I would have expected, and I am sure that I may express the admiration of builders at the skill with which the Admiralty has done this work. Mr. White has struck the true key-note of construction in the *Iris* in massed framing. It resembles the construction of the *Great Eastern*, only with intermediate framing for the local support of the skin. Notice those Z sections of steel—I wish exceedingly that ironmakers would roll them for us at a moderate rate—they have been most beneficently used there in distributing the strength of the transverse bulkheads. With reference to steel, I wish to ask Mr. White one question regarding a point which has arisen between Lloyd's and builders. I am afraid Lloyd's have got a little bit nervous lately

about steel, and are a little too anxious, and an order has been promulgated that if we work a piece of steel in the fire that it is to be annealed afterwards, and wholly annealed. I do not say that that order is a bad order, but it would have been better if it had been issued with some comment. It will immediately strike you that the most difficult subject of annealing in a ship is the beams. We weld in the beam-knees and finish them. If we are to put the beams into furnaces, we will have to expend a deal of money in making furnaces fit to hold them. Not only that, but I ask you to picture to yourself the appearance of the beam when it comes out. I would ask Mr. White to give us some information upon this point, because I believe our friends at Lloyds' are open to argument upon it, and do not wish to press a thing that would exclude steel from shipbuilding yards. We are at present building three steamers of steel to Lloyd's rules, and we find that the maximum saving we can make between iron and steel, building them to Lloyd's rules, is, roughly speaking, between 14 and 15 per cent. That is actual saving as between the scheduled weights. Mr. White says 12 per cent. It would be well that we should have a definition of what the percentage is taken on. We took it upon the gross invoiced iron and steel used in the hull. I was wondering whether Mr. White does not take it on the woodwork in addition. If he does, a totally different interpretation of the figures arises. Mr. White has invited us to compare a steamer of the merchant service with a steamer of the Admiralty service. It is with no wish to depreciate in any way any steamer of the merchant service that I wish to point out to you that I do not think the Admiralty should have taken the *Hecla* as the representative of a steamer of the merchant service. There are one or two points in her which would not be in an ordinary steamer of the mercantile service. Of course there is the greater subdivision of the hull which is due to Mr. Barnaby, which greater subdivision, I am happy to say, is spreading in this country. We are at present building three steamers subdivided in that way without regard to any special Admiralty wants, but because we believe it is good as a matter of safety. But there are points in this steamer that are not due to Admiralty influence, and which would not be found in an ordinary merchant steamer. Referring to Mr. Barnaby's remarks yesterday on the height to which the coal had to be piled up in the *Hecla* to protect the engines, anyone looking at those engines, and knowing their power, must see at a glance that it is totally unnecessary that the engines should be so high. Two cylinders will do work thoroughly economically up to 3,000 or 4,000 I.H.P., and therefore the piling up of the coal at that point is not an inherent quality of a merchant ship, because an ordinary merchant ship would not have engines with four cylinders that would require the coal to be piled up to anything like that height. Another point in which an ordinary merchant ship would come nearer the Admiralty ideal than the *Hecla* is this: if you refer to Mr. White's able *Manual of Naval Architecture* (which I think should be read by every shipbuilder in this country), you will find that one of the best portions of it is upon the question of cutting away the gripe. Mr. White informs you that by doing this you have many disadvantages for one advantage, that being increased turning power. But Mr. White has pointed out that an increased size of rudder is sufficient to attain this, and with the enormous mechanical power we have, both hydraulic and steam, there is no necessity for sacrificing the gripe. What you lose by cutting away the gripe of a ship is, first of all, form; secondly, you lose what is a most important Admiralty requirement, steadiness. The flat part of the gripe is a great help in steadying a ship. Now that I have spoken about this point, I will touch on another, in which the *Hecla* is not a fair representative of an ordinary merchant ship. It is this: if any of you will look carefully at the position of the coal and the engine and boilers, you will see that the centre of gravity of the total amount of coal in that ship must be considerably behind the centre of buoyancy, and that means that when the *Hecla* goes to sea, if she burns out all that coal, it will change her trim to the disadvantage of the propeller. That is a point which I think in all merchant steamers, or most of them, has generally been considerered. We apply a longitudinal

metacentre calculation, for which we owe the utmost thanks to Mr. Barnes, who has made it easily workable, and we place our coal in the ship so that, if possible, we shall run on an even keel throughout the voyage.

Mr. William John: My Lord, Mr. Denny referred to the centre of gravity being over the centre of buoyancy. I think he must have meant over the centre of gravity of the load water line, so that the ship should rise and fall without altering the trim.

Mr. William Denny: They nearly correspond.

Mr. William John: Another point that was referred to was the annealing of steel required by Lloyd's Registry. Perhaps I may be allowed to explain the matter. It is not that every plate and every angle has to be annealed, but when you heat one end or corner of a plate to flange it, or anything of that kind, you must heat the whole of the plate afterwards to allow it to cool uniformly. The reason for it is this. It has been found over and over again with boiler plates which have been heated at portions of their area that they afterwards, in a most mysterious way, cracked when there was apparently no earthly reason for their cracking, even when men were carrying them in their hands—they have cracked off, and the only reason I have heard for it is, that between the heated portion and the cold portion, there is set up a state of initial stress, which makes it more or less dangerous to use it unless it has been re-annealed. That is the reason why the Committee of Lloyd's have become cautious—Mr. Denny thinks too cautious, but I cannot agree with him in that opinion. I think it is a necessary caution until we know more about this material. I would refer to one or two points in the construction of the *Iris*. The *Iris* resembles in construction some of the modern merchant ships, far more than most of the ships in the Government service, but I may say at once that she is considerably lighter in scantling than a ship of her size would be in the merchant service. Her plating is thinner, and the bottom in a merchant ship of her size would probably require another longitudinal; and in addition to the bracket system, which is four feet apart, we should require an intermediate frame to give additional strength and support to the outside plating. That is necessary. It may not be necessary in the *Iris*. I do not wish for a moment to say it is necessary in the *Iris*. The *Iris* may not have to do the work, and may not have the knocking about that a merchant ship has. For instance, it is not at all an unusual thing for a merchant steamer coming out from New Orleans to bump on the bar, or to touch the ground in other places, and I think they require more local strength than is given to the *Iris*. There is another point that occurs to me, which is this—it is difficult to criticise in detail, drawings which we have only seen for a short time—but some of the Z irons, for instance, and some of the angles, are of sizes we very seldom meet with in the merchant marine; and I think we should hear a good deal of outcry if they were put forward by us, because they are not common, and we go as much as possible for common and ordinary sizes. If you go into Z irons and vary your construction from compartment to compartment, by fitting different angles at the ends of the ship and the coal bunkers, and again in the engine space, you introduce complication which leads to cost, and very heavy cost. I believe, in the case of the *Iris*, the cost of the hull was something like £90,000. Now a merchant ship of her size would certainly not cost for the hull more than about £30,000, with heavier scantlings, and therefore I cannot help thinking that the complication is unduly costly, although perhaps some complication is necessary in a war ship—at least in a ship of war that is to be subdivided as shown on this plan. But, obviously, the merchant marine could not follow a construction of that kind, which might entail double or treble the cost. I should say that possibly and probably the great cost of the *Iris* would be due in part to the fact that she was the first ship built of steel, and probably the steel was much higher in price then than it is now, so I do not wish to press the point at all as to the exact figures. But it is a fact that the complications which I see in the double bottom there, are more than we should find in a merchant ship, and I think if you

were to follow it you would run up the cost in a way that merchant ship-owners would not stand. Then another point which occurred to me when Mr. White was reading the paper is with reference to the double bottom, and the use the double bottom is put to, and it has reference rather to the design than to the construction of the vessel. We know that in a war ship, where you have to carry weights high, you must have more beam than you require in a merchant ship, and Mr. White has said in his paper that one of the reasons for keeping the beam of the *Iris* so great was to give her sufficient stability when the vessel was light. It occurs to me that the double bottom might be used with advantage as a water ballast tank for the ship when she is in the light condition, and you might be able to reduce the beam, and perhaps—I do not say you necessarily would—you might get greater speed. You might reduce the resistance, and reduce the amount of engine-power required for driving the vessel, which is a most important element, because it affects the coals and everything else. I know from former experience in the Government service the different points of view from which a double bottom is looked at. A double bottom is fitted in a Government ship almost entirely for safety. Now a double bottom is fitted in a merchant ship, not for safety at all, or at least not for safety as a primary consideration, but it is fitted for commercial purposes, for the purpose of water ballast. I know that the constructors of the Navy are perfectly well aware of the use to which water ballast can be put. But it is well known that in the minds of officers of the Royal Navy there is a dislike to the use of water ballast, and I do not think that water ballast has ever been used in a Government ship as it ought and might be used, and as it is used in the merchant marine. There was a prejudice against water ballast in the merchant marine, but the commercial advantages of it were so great that owners compelled their captains to use it commonly and frequently as a matter of course, and the prejudice in the minds of naval officers, so far as the merchant marine goes, against water ballast is entirely swept away. I think it might be introduced into the Royal Navy, and that some very valuable advantages might be gained by a more extended and liberal use of water ballast, by their availing themselves of the double bottoms which have been fitted for the purposes of safety and for other purposes.

Mr. J. D'Aguilar Samuda, M.P., V.P.: My Lord, I regard the paper which has been read by Mr. White upon the *Iris* as one of the most valuable acquisitions this session. We can scarcely have a more important subject brought before us than is dealt with in this paper—that is, the introduction of steel upon a large scale in the marine, in this instance in a shape which might be a type to start with, both for commercial as well as for Admiralty purposes. I am one of those who believe, and have for a long time believed, in the substitution of steel for iron. Anything which brings that carefully before our consideration, and especially anything which brings so excellent an example as this before our notice, must have wide and beneficial results. Now I may be permitted in starting to say that generally I approve entirely of the description and construction and the arrangement of this ship. But I want to point out differently from that which appears to have fixed the attention of Mr. White, and certainly in opposition to the observation of the last speaker, that I do not think steel has now, or ever has had, a fair consideration at the hands of Lloyd's. I believe that Lloyd's have never treated it in a proper way, and I believe they at the present moment do not at all realise the advantages the public are being deprived of by the course they are taking. Now I begin by stating that as long as twenty years ago I was engaged in building steel ships, and at that time one of the conditions which was laid down by the Company I was engaged for, which was a general one, ranging over a large number of vessels, was that I was to obtain Lloyd's certificates for the ships I was building. A case arose in which it was impossible to carry out the conditions that were required unless steel was used. I did not hesitate to use steel, though I had to pay for steel at that time £50 a ton, and in some instances even £60. But when I went to Lloyd's to enable me to give effect to the classifi-

cation condition I had entered into, and without considering that this trouble would come upon me, they resisted the matter altogether, as being wholly and entirely beyond their province. They would do nothing with it, and I think I am not misrepresenting them, when I say, that the conditions they wanted to impose upon me would practically have deprived me of the power of building a ship at all. I think I am right in saying that they wanted to impose on me the necessity, although I should not like to be positive as to it now, of putting exactly the same thickness of steel that they in their rules at that time were using for iron. Of course I was compelled to disregard Lloyd's altogether, and I arranged to build the ship without. But Lloyd's have acted on a wrong principle altogether, and they are acting now with steel vessels upon the same principle. Now the wrong principle they are acting upon is this: ever since they have been able to grow in the knowledge of building iron ships, they have always laid down their rules from time to time to meet the case of the worst description of iron being used in their ships, and they have never made any difference, nor realised that they might have had a very considerably better state of things existing in the mercantile navy if they, instead of depending upon brute strength—I will not say brute strength, but on thickness, which did not give them the brute strength they required—had made their rules compatible with improved quality. I do not hesitate to say that was at the root of all mischief that Lloyd's began with, and are keeping to at the present time. They go upon this principle: that as people, for the sake of decreasing the price, will accept what is called ship plates—the most damaging name that ever was, and the most damaging material that ever was used in ships, and that never has been able to find its way into my yard, or, I believe, anybody's yard who has any regard for the quality he puts into his ships—they take that as their standard, they make every good ship-builder increase his thickness, until he comes up to the thickness they require to be used of this bad quality of iron, and then, when you come to steel, they make a reduction in thickness in steel which they think is sufficient, taking it in comparison with this inferior iron. In this way they at the present moment make some rule—I think I am right in saying they will not allow more than a reduction of one-fourth in the steel compared to that which they have in iron; and compared to the stuff which they allow to go in and be called iron, one-half would be stronger of the good cast or Landore steel made at the present day, but compared with the good iron which people who know what they are about desire to use in their ships, there ought to be a reduction of at least one-third. I do not at all wonder that in vessels of this description they are not wishing to go too much in advance of the time, and that those who are responsible for the construction of the *Iris* and *Mercury*, might in some degree have limited their steel in thickness to approximate with Lloyd's rules. But, passing from that, I would venture to say there are some other things in connection with this vessel which I wish to speak about. I want to allude a little to the process of annealing that Mr. Denny called your attention to. This annealing process, again, is put forward in a manner, and with an authority, which I venture to think Lloyd's have no right to do. My impression is that all institutions like Lloyd's are bound to make themselves inspectors, and not directors. It is the greatest mistake, and the greatest misfortune, for naval architecture when these people set themselves up as teachers. Now they talk about annealing. In all the vessels I have alluded to, which were built twenty years ago, and are going at this present day as well as they did at first, very little heating, and no annealing, was used. To talk about annealing beams is the most absurd thing that can be supposed. If a beam was attempted to be annealed, instead of curving in the direction of the deck, you would find it curving in exactly the opposite direction. In bending beams they are bent in the opposite direction to that which they are required to finish in, due allowance being made, because by observation and practice it is known they will take the opposite form, being pulled so in consequence of the difference in contraction that takes place at the bulb and thin edge. These you

are to put in the fire, and destroy that which you have carefully arranged. It is too ridiculous to talk of. Then I wish to say a word with regard to the observations which have been made about double bottoms. I do not agree with them at all. In the first place, allow me to say that there is a great mistake in supposing that advantage is not taken of the double bottoms in ships of war to make the double bottom water ballast carrying tanks. I have myself built a very large number of iron-plated ships, and in every single instance they have been made so that they can be used as water ballast tanks, and in making their journeys from here to the different countries they have been built for, they have almost invariably been used as water ballast tanks. But I consider the double bottom is an extremely scientific and good arrangement. You do not increase the weight you use in the ship in any degree whatever comparable with the strength that you get by introducing it, and therefore it is a movement in the right direction; but again, if it appears with Lloyd's to clash with their rules, I should not wonder if you find that it is interfered with, and they will either resist it altogether or attempt to make you subscribe to some rule, so that you shall not have their classification if you adopt it. Now I turn from this, which is not a pleasant subject, but I think it is so important that I venture to press it on the meeting, because I think it is high time that constructors were left a great deal more to themselves to produce what they think right, and that Lloyd's should confine themselves to their legitimate province of approving what is good, and not teaching people how to make it different from what they desire to make it themselves. I want now to call Mr. White's attention to one matter which is not very important, but I do it with a view of affording the information, if he desires to have it. He says, "No sea-going steamer having realised the measured mile speed of the *Iris*, I cannot illustrate the matter by comparing their performances with hers." If he does not know it, I shall be very happy to give him the particulars. I myself more than fourteen years ago built a vessel of the same size as the *Iris*, or larger even, which has gone at greater speed than the *Iris*, and I shall be happy to furnish him with full particulars so that he can make his calculations and add them to these valuable notes which he has been able to lay before us. The name of the vessel is the *Mahroussee*, a vessel built for the Viceroy of Egypt, and she attained on a measured mile something like 18 and 6-10ths knots—that is more than the speed which he speaks of.

Mr. N. Barnaby, C.B., Vice-President: My Lord, I think the difficulty with regard to the annealing of steel may perhaps be got over in the case of the beams if the builders will be good enough to work the beams cold. They will find, as we have found, that steel will submit to a treatment cold that iron will not submit to. We have, for example, found that angle bars of steel which were rather harsh and were giving us trouble, nevertheless submitted to severe joggling cold. It is usual, as everyone knows, in the case of iron, to make them hot for that purpose, but our own practice has been whenever we have put hot work into steel to anneal it. What I want to say is, you will find if you take pains that you will want very little hot work indeed; that your plates, your bars, your beams—everything you have to do, if you like, can be treated by you without your ever putting it into the fire at all; and that is one of the greatest advantages of steel. I must say I think Lloyd's are going cautiously in insisting on this, that if you will put hot work into the material you should for the present anneal. In the case of the beam arms I think, if Mr. Samuda would make an experiment, he would discover probably that even if you did it hot, which certainly you need not, the amount of injury done is so slight that it would not need to be annealed. That is easily proved, and need not present any difficulties, as it seems to me, between Lloyd's and shipbuilders. I should like to say one word with regard to a most important question—namely, the cost of a ship like the *Iris*. It has been very rightly said that no merchant shipbuilder could undertake to build ships in the way in which this ship has been built; but her great cost is not due so much to the kind of work which you see in the bare hull, it is due to the fittings: and I can appeal to Mr. Harland's experience

in that matter with the *Hecla*. The *Hecla* was bought by the Admiralty for a sum, including her engines and an outfit, not exceeding much the cost of the hull of the *Iris*, but he had to put a certain amount of work in, to bring her up in a part of her hull to the completeness required in a ship of war; and he well knows how costly it was. If the *Hecla*, finished as she would have been finished by Mr. Harland, had been sent to a Royal dockyard, they would have said, "You have sent us a mere shell." As compared with other merchant ships, she was a splendid ship for her work. In a ship of war, if you go between decks you will see where the money goes. There is no end to it. No one can cry about it as I do, but I cannot see the way out of it. People invent a new mode of attack, and we are obliged to invent a mode of defence, and the new mode of attack we are also obliged to carry. Therefore the ship must always have in what she had before, and we must take the new thing also. Perhaps it may come some day, but at present I do not see how the end of the great expenditure—not on structure or on hull, but on what we call hull; because it is for things connected with the hull, and which are said by naval officers to be indispensable for the efficiency of the ship and their own proper accommodation—is to come. It may be so, but it is difficult for a shipbuilder to understand; and I envy those gentlemen who can produce those simple and beautiful structures which we hear about, for next to nothing.

Mr. E. J. Harland: My Lord, I have very much pleasure in corroborating the remarks which have just fallen from Mr. Barnaby with reference to the excessive cost for fittings necessary for the various, and I think I may say, multifarious, requirements of Her Majesty's service. With reference to the hull alone, the hull in the merchant service, as Mr. Barnaby has said, may be everything that is desired, but on that has to be grafted an immense number of fittings besides what has hitherto been in use in naval warfare. But now the introduction of the torpedo has introduced a very expensive adjunct. A remark was made in the early part of this morning's discussion with reference to the measured mile. I would simply say that I agree entirely with my excellent friend from Scotland, that for many years the measured mile was not known in Scotland; it was a distance of sixteen miles on the Clyde, which was a very wholesome run indeed. I could find no fault whatever with that distance. Occasionally, the vessel might be brought round a little earlier than in strictness they were entitled to do, but that was the only little bit of jockeying that was introduced. I feel that it would be unfortunate in a scientific institution like this, if in our meetings we were to be rather disposed to congratulate than to criticise, and perhaps severely, because a severe criticism, within the range of polite manner and gentlemanly feeling, is the very thing that should be courted here, and therefore I have no disposition whatever to place more credit to those gentlemen who have given so much time to the writing of these papers than they deserve. They deserve an immense amount of credit, because they have evidently exhausted the subject as far as it can be exhausted; but I feel in an Institution like this, where the mercantile marine ought at any rate to occupy a very important part of the time, it is not perhaps well for us that we should dwell too long or have too many papers on naval construction—that is, for war purposes—because, after all, the backbone of the country is not our war-ships, I hope, but our merchant ships. Now, some reference was made to the *Hecla* as being a very exceptional merchantman. If the *Hecla* had been produced some years ago she would have been considered a marvel. I might almost say that we have been brought to see, within the last few years, that what was once considered to be an anomaly in shipbuilding, what were called "coffins," have since proved not "coffins," but the life of the commerce of the mercantile marine. I am referring to the long ships. Now the difficulty which long ships had to contend with at first was what was called their unhandiness. The unhandiness of these long ships is a pure myth. If you ask any pilot going into Liverpool accustomed to these long ships of the modern type, he will tell you he would rather swing one of these ships 400 or 450 feet in length, with the fore-foot cut off, and with of course a whole-

some size of rudder, in the tideway of Liverpool, with the river crammed full of shipping, than one 100 feet shorter without the forefoot cut off. As to the "grip" of a merchant steamer's bow, you want nothing of the kind. The grip may be wanted in a sailing vessel, where leeway is a question for consideration. But what do you want with grip in a steamer? The flat side of her is grip enough. They know nothing of leeway, and a little bit of canvas spread on her, of which the *Hecla* is a very fair type, is not sufficient to give them any leeway. Then as to the cutting off: it is an advantage in two ways, quite independent of the matter of steering. Now, take the *Iris*. I do not wish to draw attention to that particular ship, but take the forefoot of any modern shaped steamer, and I undertake to say that if you cut off so much of the forefoot as is represented on the *Hecla*, you will find that you cut off an amount of displacement which would not equal one-fourth or one-fifth of the amount of dead weight on that forefoot. In other words, when we go on board a merchantman what do we like to see? In the rapid Holyhead steamers the first thing the captain did when he went aboard was to shift all his anchors off the forecastle and get them as nearly amidships as possible—and why? Because the paltry three or four tons of weight on the nose of the ship was quite sufficient in driving them at a high rate of speed in a head sea, for the captain to know perfectly well, that that amount of dead weight on the very nose of the ship was objectionable. Therefore he ingeniously and properly shifted the anchors to the after-end of the turtle-back, and arranged for them to be let go there. There is far more weight cut off the forefoot of the *Hecla* than the weight of both her bower anchors. Furthermore, if you take the fine entrance of the lower part of any merchant steamer—if you look to the lines of that part of the ship, you will at once say that they are needlessly sharp. You have not made them sharp for satisfaction or for beauty, but it is merely in order to get a sufficiently fine load water line; and still sticking to that forefoot being brought down, you are inevitably brought into extremely fine lines at the forefoot—so fine that I say if you only simply cut that piece off you are removing a useless piece of matter from the bow of the ship which is unnecessarily fine for cleaving the water, and is only so much iron to be carried in the most objectionable part of the ship, the bow, when she rises to the sea. With reference to the coal bunkers, Mr. Denny, who favoured us with so many very lucid remarks, is perhaps not aware that the *Hecla*, like many others of the same type of ship, is so arranged that she has what is called a coal-hole forward of the coal shown in the bunkers. Those bunkers shown are merely what are called the bunkers proper, but there is a coal-hole forward of those bunkers which will contain as much coal as will take that ship out to New York without using a single ton of coal out of the bunkers. The consequence is that when she goes on her voyage across the Atlantic, or a similar voyage, she lightens herself forward. We must not take the weight of the bunkers as proper for the centre of the vessel, we have to take the whole thing. We take her so that on her outward voyage, taking her own coal out, she lightens forward. That is a very important circumstance. In coming home, as the cargoes pay very much better homeward than outward, she utilises the coal-hole and other places for cargo. On the return voyage, whilst she has the disadvantage of slightly lightening herself aft, she has the great advantage of the average of westerly winds in her favour. The consequence is that, like all things in Naval Architecture, if the Naval Architect enters his profession and follows it out on scientific principles simply, he will make a great mistake; and if he follows it out from a shipbuilder's point of view, he will make a mistake. He has to be scientific, and he has to be practical, and he has to be something of a merchant. In other words, the result of the whole thing must be satisfactory to everybody, and particularly to the man who pays for the vessel and owns her. We must not lose ourselves in science. I should be the last man in the world to say a word against it, but I think at the present time we are disposed to make too much of what is called education. We are educating our children now above what the probability is they will ever be able to utilise in after life. A shoemaker educates his child

to be a doctor, or a barrister, or something advanced, in place of teaching him how to make the very best shoes. I contend that the sons of shipbuilders are over-educated, and are apt to lose themselves in figures, because they are very apt to follow the easier course of study, having books and papers before them, and neglect the workshop, and neglect coming in contact with those merchants who have to consider most carefully what class of ships will pay the best. I say if a shipbuilder does not combine in his profession all these varied and sometimes opposing forces, he will make a serious mistake, and lower the value of his profession not only as a profession, but also the value of it to this commercial community.

The President: The clock warns me that it is almost time we should bring this interesting discussion to a close; but I cannot call on Mr. White to reply to what he has heard, without offering him my congratulations on the discussion which his paper has elicited, which I think must have been gratifying, and very justly gratifying to him. I wish also so far to intrude upon you in point of time for one moment as to say that I entirely concur, as far as my own individual opinion is concerned, with regard to the immense importance of our mercantile marine which was so justly referred to by Mr. Harland, in the able speech which he has addressed to us; but I think Mr. Harland would admit that in these times of transition in Naval Architecture, it is only natural that an Institution of this kind should devote its attention to the construction of ships of war, and I hope he will also join in the opinion which I myself entertain, that we cannot come here and discuss paper after paper day after day on the best mode of constructing our ships of war, and of varying the construction, without touching on scientific principles which bear not so directly but most importantly on the interests of the mercantile marine. I hope that I have not said anything unpleasant to Mr. Harland in making these observations upon his able speech, and I hope he may agree with me that in such a discussion as we have just heard, we are really promoting the interests of Naval Architecture at large, whether applied to merchant ships or ships of war.

Mr. W. H. White: My Lord, Mr. Harland thoroughly expressed the feeling which I have when he said that in an Institution like this we should have more papers bearing on the mercantile marine. I think the Secretary will bear me out in the statement, that this session there has been great difficulty in getting any such papers; and I may say in personal explanation that I should have been very glad, pressed as I was with other work, not to have undertaken the preparation of this paper, but when it was found in preparing the programme that gentlemen from the mercantile marine did not come forward, then the Admiralty and Lloyd's were applied to to see if papers could be obtained to give some information of general interest. I should be only too glad to have the opportunity of studying in detail the performance of those wonderful vessels which Mr. Harland's firm has constructed. I have often thought, if the performances of the series of vessels of the *Illyrian* type, of which the length was gradually increased by adding certain proportions of the beam, could only be put down in detail, we should get a complement to Mr. Froude's experiments on the effect of straight of breadth which would be most instructive. Once or twice I have been on the point of writing to Mr. Harland myself, to ask for the facts, but I have never yet mustered the courage to do so, but I will accept with gratitude Mr. Samuda's offer of the particulars of that very fast ship he spoke of. I believe I am right in saying she was a yacht, and of course in that respect differed from the *Iris*, which is a self-sustaining ship of war. I do not wish to raise the question, but I shall be most grateful to Mr. Samuda for the data if he will give me them. Then with regard to Mr. Thornycroft's remarks on torpedo boats, I was thoroughly aware of their enormous steaming capacity at moderate speeds, but what I meant when I said that they were not sea-going was in the sense of their being self-supporting, carrying an equipment such as the *Iris* would, which is a vessel which could be absent from port for many months, except when requiring coal. That is the sense in which I meant sea-going,

not simply as measured by the power of steaming long distances at moderate speeds. I am fully aware of what Mr. Thornycroft has achieved, and I admire most thoroughly what he and Mr. Yarrow have both shown can be done as to high speeds in vessels of a small size. Mr. Denny's question as to steel Mr. Barnaby has answered, and I think Mr. Denny was satisfied with that explanation.

Mr. W. Denny: There is one difficulty, which is this, that in turning down the end of the beam you require to weld in a piece.

Mr. White: Perhaps I may be permitted to add to Mr. Barnaby's explanation by saying that our own practice has not been to anneal the whole beam in all cases. That is the point which you are more particularly considering. Then as to how the percentages were taken for the saving in weight of the hull. Mr. Denny was quite right in supposing that I expressed the percentage on the footing of the total weight of the hull. Mr. John remarked that by using water ballast in the double bottom perhaps there might have been a diminution of her resistance if her breadth were decreased. I only wish that Mr. Barnaby, the responsible designer of the ship, had said what the history of the design was; but, as a matter of fact, the *Iris* had her beam increased for the purpose of lessening her resistance, and she gained in stiffness, because of the proportions accepted to decrease the resistance—that is to say, for stiffness we could have accepted a less extreme beam than was adopted for the purpose of lessening the resistance. That is the matter of fact. The *Iris* as she stands is a stiffer ship than one might expect. There is no doubt she has a metacentric height in a loaded condition of over 3 feet. I want to draw attention to the fact that in the Paper I speak of the stiffness as it affects the margin of safety—the ship has to be knocked about and partly damaged, and the better she starts in stability the better chance she has of sustaining stability. Now one thing more. The *Iris* is a ship that has to turn at a great speed. Anybody who has looked into the question of turning at a great speed will know that as you increase the speed of the ship with a good metacentric height, you get a considerable angle of heel in the turning. That is another reason why the *Iris* should have good beam. The use of double bottom spaces for water ballast in the *Iris* would be of no service. We do not want the water ballast. We have the means of using it in the *Iris*, because we have a system of flooding and pumping out the double bottom, and if it were needed it could be used for the purpose of trimming or stability. Then as to the cost of hull, Mr. Barnaby has fully explained that, with one exception. He did not mention a thing which Mr. John himself mentioned, and it is an important matter—that is, that the price of mild steel now is very different from what it was when the steel for the *Iris* had to be purchased. In fact, the mercantile marine is now reaping the benefit of the policy which Mr. Barnaby so ably advocated here some five years ago, which led to the use of steel in the hull of the *Iris*. As to the angle bars (and this is a matter of detail), Mr. John properly pointed out that these bars are of large size. We have, in fact, greater choice of size in the Royal Navy than perhaps the mercantile shipbuilder would have. We have adopted the system, as regards both steel plates and bars, of specifying the weights by either units of length or units of area, and the manufacturers work to these units with the greatest ease. Of course with the same sized flanges you may have angles rolled thinner or left thicker, and so get a varying weight per foot. Lastly, as to the resemblance in structural arrangements between the *Iris* and the modern merchant ship, I think that is a matter for congratulation. It shows that those who have given most attention to the matter, both in the Admiralty and outside it, have come to the same conclusion—that if you want to get the greatest advantage out of steel that can be got from it, you must use the stronger material in less thickness and prevent buckling and local strains by an efficient means of support. I think, my Lord, those are all the points which I wish to mention. I am extremely obliged for the kind opinions which have been expressed on the Paper, and I hope it may be of some service.

ON THE MINIMUM AREA OF BLADE IN A SCREW PROPELLER NECESSARY TO FORM A COMPLETE COLUMN.*

By James H. Cotterill, M.A., F.R.S.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

In a paper read before this Institution in 1865, the late Professor Rankine gave a complete theory of the screw propeller, based on the supposition that the form and dimensions of the blades of the screw were such as to throw back a complete column of water. The distinguished author believed that such was actually the case in well-proportioned screws, and no doubt, the fact that, in many cases, the thrust depends not to any great extent on the actual area and form of the blades, but mainly on the disc area, lends strong support to the supposition.

The verification, however, of the theory in the case of the *Warrior* contained in the paper in question, can hardly be considered satisfactory, since it involves the difficult question of the augmentation of the resistance of the ship due to the action of the screw, and, besides, makes assumptions as to the motion of the water at the stern of the ship, which subsequent investigation has not confirmed. Experiments made since 1865—I refer especially to those of Mr. Isherwood—had, moreover, led me to believe that the column of water thrown back by a screw is rarely complete, and it was, therefore, with great pleasure that I found that, in a paper read last year, Mr. Froude had made a new departure in the subject by comparing the action of a screw to that of a plate advancing obliquely through the water. The theory of Rankine also compares the screw to an oblique acting paddle, but, instead of making use of our independent knowledge of the pressure on the blade, it proceeds by considering *a priori* what the change of momentum and quantity of water acted on must be, and fails if that water does not occupy the whole disc area.

Mr. Froude's theory of a revolving plate, if applied to actual screws without any qualification, would lead to the conclusion that the thrust of a screw of given speed and slip, might be increased indefinitely either by diminishing the pitch or by increasing the blade area, results which cannot be accepted. It becomes, therefore an important question, to know what are the limits within which either theory is applicable, and to which theory the action of an ordinary screw propeller most closely approaches. Without attempting a

* The column is, for brevity, throughout this paper said to be "complete" when all the water passing through the space swept out by any element of the blade is thrown back with the same velocity.

complete investigation, it is in the hope of throwing some light on this question that the present paper has been written.

In passing from the simple plate to the screw blade, we encounter three distinct difficulties: (1) The change from motion in a straight line to motion in a curve; (2) The change from a flat surface to a curved surface; (3) The limitation of the quantity of water acted on.

In the straight line motion, the water is disturbed at some distance ahead of the plate, whereas when the direction of motion is constantly changing, this can hardly be conceived to be the case. Hence the disturbance produced by a plate which moves in a curve, resembles that caused by a body when first set in motion through water, and the pressure on the plate must be altered (possibly increased) by the curvature of the path. It is probable, however, that the difference is not important, at any rate for small values of the angle (ϕ) between the plate and the direction of motion, since otherwise the thrust of a screw of given total blade area, would depend on the number of blades, to a much greater extent, than appears to be actually the case when all other things are equal.

Under the same circumstances, the curvature of the plate also cannot much affect the pressure. For let us suppose the plate of very small depth perpendicular to the plane in which the centre moves, and let us imagine that plane folded round in a cylinder of given radius, so that the centre line of the rectangle becomes a spiral traced on the cylinder, while the plate itself becomes an element of a screw blade of given pitch. The centre of the plate then traces out a spiral path on the cylinder, which will be inclined at the small angle (ϕ) to the element of the blade. The paths of the particles of water relatively to the element will be similar spirals when undisturbed; while near the element they will deviate from such spirals, nearly in the same manner, and to the same degree, as the plane paths in the plane motion deviate from straight lines. Hence the quantity of water acted on, the change of momentum of that water, and consequently the pressure on the element, must be nearly the same in the two cases. A complete screw blade can now be built up from the spiral elements, and the thrust determined by summation.

The third difficulty, that the quantity of water acted on by the screw is not unlimited as in the plane motion, but cannot exceed that which passes through the screw disc, is much more serious than the other two, and compels us to restrict the theory to screw blades of small dimensions as compared with the diameter. For in order that the plane motion may exactly correspond to the spiral motion considered above, we must consider, not a single plate, but an infinite number of plates, ranged along a line inclined to the plate at an angle (θ) equal to the pitch angle of the spiral motion, and placed at a distance apart equal to the circumference of the cylinder, if the screw have but one blade, or at the distance of consecutive blades when there are more than one. The pressure on each plate will now be less than that on the single plate, because part of the water which in the single plate is disturbed by its action, is now intercepted by the plates which lie in front of it; and thus it appears to me that the pressure on a screw blade must be less than that given by Mr. Froude's method, because part of the water which would otherwise strike the blade, and which is necessary to develop its

full thrust considered as a revolving plate, is intercepted by the blade which lies immediately in front, or if there be but one blade, by that blade itself in the course of the revolution immediately preceding. The magnitude of the deduction will be greater, the larger the blades, and some idea of the size at which the deduction becomes sensible may be gained by the following rough method.

Let l be the length of the plate, V its velocity in feet per second, then the pressure on it per foot depth is

$$P = 1.7\ l \sin\phi\, V^2.$$

Now imagine each particle of water to receive the same change of motion V sin (ϕ), perpendicular to the plate which the particles in contact with the plate actually do receive, and let b_0 be the breadth perpendicular to the plate which those particles would then occupy, or, as we may conveniently call it, the "breadth of uniform disturbance;" then, disregarding the angle (ϕ) as always small compared with (θ),

$$Q = b_0 V$$

is the quantity of water acted on per second, while the pressure on the plate becomes, with feet and seconds as units,

$$P = 2\, b_0 .\, V^2 \cdot \sin\phi, \text{ nearly};$$

comparing which with the former value of P,

$$b_0 = .85\, l,$$

showing that the breadth of uniform disturbance is . 85 the length of the plate. The actual breadth of disturbance is of course much greater, and hence it follows at once that if the perpendicular distance of two plates be equal to . 85 l, a good deal of the water which would otherwise strike the rear plate, will be intercepted by the front one, and the pressure on each one of the row of plates, will be sensibly less than is given by the formula.

To transfer this to the case of a screw, let b be the aggregate length of the circular arcs formed by the projection of the elements of given radius, of all the blades on a thwartship plane, and let C be the whole circumference, then C corresponds to the distance between the plates measured along the line on which they are ranged, while for l we must take b sec θ. Hence we infer that the blades will considerably interfere with each other if

$$C \sin\theta = .85\, b\, .\sec\theta;$$

or,

$$b = 1.18\ C \sin\theta \cos\theta$$

In order, therefore, that the theory of a revolving plate may be applicable without sensible deduction, the fraction of the circumference occupied by the blades must not be greater than 1·18 sin θ cos θ, or more probably considerably less. This explains why Mr. Froude's formula for the thrust of a revolving plate gives an infinite result, when the pitch, and therefore the pitch angle θ, is diminished.

And if we increase sufficiently the fraction of the circumference occupied by the elements, I think it is clear that the resulting motion of the water must be nearly such as is supposed in the earlier theory of Rankine. For if the row of plates supposed above, are placed near enough together, all the water will move with the same velocity after passing through; that is

to say, in the case of the screw elements, a hollow cylindrical race will be generated, the velocity of which will be found, as Rankine finds it, by supposing the velocity parallel to the element unaltered, and the velocity perpendicular to it destroyed. It is true that if we followed the water which strikes any element, we should find that some of it passes on the leading side, and some on the following side of the element, and that therefore the velocity of some particles of water, parallel to the element, is nearly reversed while that of others is increased. This has considerable influence on the frictional resistance (see Appendix), but since the momentum parallel to the element cannot be altered, the change of momentum must be the same as Rankine supposes, and hence the thrust must be nearly the same. I see, therefore, no reason to alter the opinion I have elsewhere expressed,* that this theory, though not mathematically exact, is very approximately so when a complete column is formed, and I think it applies to this extreme case, with about the same accuracy that the theory of a revolving plate applies to the other extreme case, where the screw blades are supposed very small.

From what has been said it appears that the thrust of a screw will be less than that given by Mr. Froude's method, by an amount which will be greater, and greater as the column approaches completeness, and at length, when the column is completely formed, becomes that given by Professor Rankine's method, after which it can increase no further. If then we equate the values of the thrust of a spiral element, furnished by the two theories, we shall find the minimum length of the element, which will be capable of generating a complete column. This length is expressed by a fraction, which gives either the fraction of the pitch occupied by a projection of the element on a longitudinal plane, or the fraction of the circumference occupied by a projection on a thwartship plane. In the Appendix it is shown that this fraction is given by the formula,

$$\text{Fraction of pitch or circumference} = 1.18 \frac{(q^2 \times 1 - s)^2}{q^2 + (1 - s)^2} \cdot \frac{q}{(1 + q^2)^2},$$

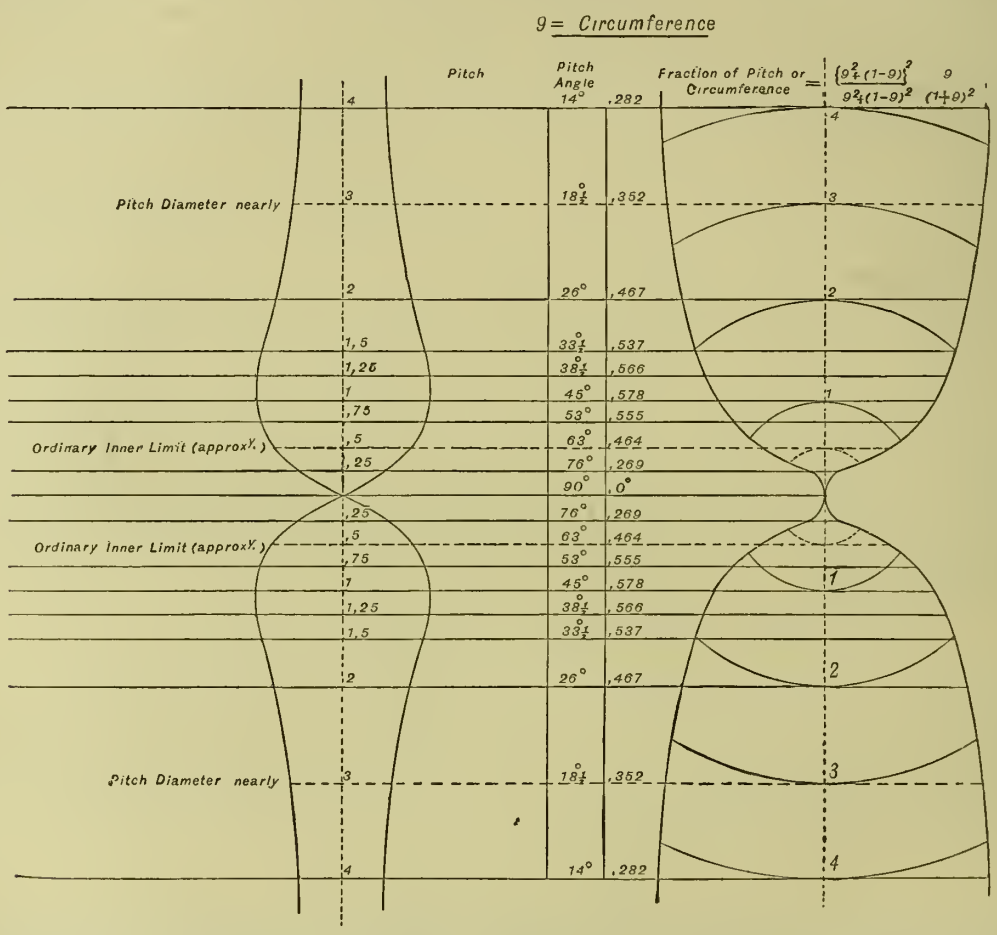
where q is the ratio of the circumference to the pitch, and s is the slip expressed as a fraction of the speed of the screw. For reasons which I need not stop to point out, when $s = 0$ we get the formula given above for the value of the fraction, at which the blades interfere with each other considerably.

Numerical calculation from this formula, for various values of q, gives the value of the fraction for each element of the blade, and the results may conveniently be exhibited graphically, the several lengths and arcs, when set out at the corresponding radii, forming the longitudinal and thwartship projections of a screw blade, which everywhere satisfies the condition in question. The diagram shown is adapted to the case where the slip is 25 per cent. and the pitch uniform, but the results are nearly the same for all slips between zero and 50 per cent. The numbers given on the diagram are the values of the fraction found from the formula, and refer to the aggregate length of the blades, which theoretically may be divided between any number of blades arranged in any manner: in the diagram the screw is two-bladed, and the values of q range from 0 to 4. In practice, q would not be much greater than

* See "Annual" of the Royal School of Naval Architecture for 1873.

3, or less than ·75, limits which are marked on the diagram, and which show the part of the ideal surface, which corresponds to an actual screw blade of pitch about equal to diameter.

We have thus determined the form and dimensions, of the smallest screw blade, which could under any circumstances form a complete column, but it does not follow that such a blade actually would do so: it is more probable from what has been said, that the completeness of the column increases but slowly with the size of the blade, as it approaches the limit. It appears to me, however, that the diagram furnishes a sort of rough indication, of the degree



of completeness, in which the column will be formed by each portion of an actual screw blade, and hence of the degree the action of the actual screw, approaches either of the two extreme cases considered above.

On examination of the diagram, it will be seen that the form is intermediate between the common and the Griffith screw, but that the dimensions are much greater. Like the Griffith, the length of blade is a maximum for a pitch angle of about 45°, but the aggregate length is then nearly ·6 of the pitch, or fully 50 per cent. greater than that of the actual screw. On the other hand, the aggregate length at the tips of a screw, the pitch of which is equal to the diameter, is shown on the diagram to be ·35, which differs little from that of a common screw.

It thus appears that ordinary screw propellers do not form a complete column, and that the only case in which they approach doing so, is in the outer part of a common screw; and as there is reason to believe that the efficiency of a screw is increased, by diminishing its length at the tips, it is probable that too great an approach to completeness is not favourable to efficiency: the reason of which may be conjectured to be, that in that case, increase of the blade area increases the thrust much less than it increases the frictional resistances. Another cause, however, may, and I believe often does, diminish efficiency when the column approaches completeness, and that is the augmentation of resistance of the ship, due to the action of the screw, which may easily be imagined to increase as fast as the thrust, when too much water is required to supply the screw.

It will now be seen that in actual screws, the thrust given by Mr. Froude's method must be much less than that given by Professor Rankine's, while the actual thrust is probably less than either: the deficiency in the second case for any part of the screw surface, depending as it does on the completeness of the column, will be roughly indicated by the approach of the length of that part to the length of the corresponding part of the blade of minimum area. Considered as a method of determining the thrust approximately, it is evident that Mr. Froude's process is to be preferred, and its errors may in many cases be small. It would be foreign to the purpose of this paper to enter on the question of frictional resistances.

In writing this paper I have purposely confined myself to theoretical reasoning, and have not attempted experimental verification. Hardly any experiments have hitherto been published which can usefully be compared with theory, in the present state of our knowledge, for either they involve the disturbance of the water by the passage of the ship, or they are made on screws stationary in the water in which they rotate, in which case the conditions are wholly different. Neither of these more complex cases are likely to be successfully dealt with, till the more simple case where the screw acts on undisturbed water has been more thoroughly studied.

APPENDIX.

Note I.—*Comparison of Thrusts.*

Let θ be the pitch angle, p the pitch, and t the (small) depth of a spiral element of a screw blade, r the radius of the cylinder on which it lies, q the ratio of the circumference to the pitch, then

$$q = \cot\theta = \frac{2\pi r}{p},$$

The thrust by Rankine's theory will be

$$T_1 = 2\pi m r t v (v - V),$$

where V is the speed of the ship, v the sternward velocity of the water after leaving the screw, m the mass of a cubic foot of water. Now if v' be the speed of the screw, s the slip expressed as a fraction of that speed,

$$v = v'(1 - s.\sin^2\theta);$$

$$\therefore\ T_1 = 2\pi m r t . s . v'^2 (1 - s.\sin^2\theta).\cos^2\theta,$$

which, by expressing $\sin^2\theta$ and $\cos^2\theta$ in terms of q, becomes

$$T_1 = 2\pi m r t . s v'^2 . \frac{q^2(q^2+1-s)}{(1+q^2)^2},$$

Again, let b be the length of the arc, formed by a projection of the spiral element on a thwartship plane, or if there be more blades than one, the aggregate length of all such arcs, P the normal pressure, and T_2 the thrust of the element considered as a revolving plate; then

$$T_2 = P . \frac{\text{area of projection}}{\text{area of element}} = 1.7\, b\, t \sin\phi\ V^2,$$

where V is the velocity of the element in its spiral path, ϕ the angle that path makes with the element. Now ϕ is given by the formula,

$$\tan\phi = \frac{s\tan\theta}{1+(1-s)\tan^2\theta} = \frac{s\,q}{q^2+1-s},$$

which shows that for moderate slips, ϕ is always a very small angle, its maximum being for $q = \sqrt{1-s}$, when

$$\tan\phi = \frac{s}{2\sqrt{1-s}},$$

For example, for a slip of 25 per cent. ϕ is less than 9°.
Hence we may safely write $\tan\phi$ for $\sin\phi$, and further

$$V^2 = V^2 + v'^2 . \cot^2\theta = v'^2\{q^2+(1-s)^2\},$$

whence by substitution for $\sin\phi$ and V^2, we find

$$T_2 = 1.7\, b\, t\, s\, q\, v'^2 . \frac{q^2+(1-s)^2}{q^2+1-s},$$

a formula which gives the thrust of the element in terms of the same quantities as before.

Equate now the values given by the two theories, remembering that in the second, the units are feet and seconds, and that therefore we must write $m = 2$ in the first result: then we find

$$b . \frac{q^2+(1-s)^2}{q^2+1-s} = 2\pi r . \frac{q^2+1-s)}{(1+q^2)^2}\ q,$$

which may be written

$$\frac{b}{2\pi r} = 1.18 . \frac{(q^2+1-s)^2}{q^2+(1-s)^2} . \frac{q}{(1+q^2)^2}$$

which is the value of the fraction of the circumference, occupied by the elements, when the thrusts are equal.

When $s = 0$ we get

$$\frac{b}{2\pi r} = 1.18 . \frac{q}{1+q^2} = 1.18 \sin\theta\cos\theta.$$

Now in the text this same result was obtained on the supposition of uniform disturbance just extending from one blade to the next, and disregarding . The process in this note is the equivalent calculation when ϕ is not disregarded, that is, when the slip is not neglected.

The following table shows the values of the fraction, or what is the same thing, the aggregate lengths of blades of pitch unity, when the condition is satisfied at all points of the blade for a slip of 25 per cent. Variation in the slip alters the result very little except for smaller values of cot θ than can be used practically:—

SCREW BLADE OF MINIMUM AREA.

θ.	$q = \cot\theta$.	Fraction.	θ.	$q = \cot\theta$.	Fraction.
14°	4	·282	45°. 40′	·988	·59
18½°	3	·352	53°	·75	·555
26°	2	·467	63°	·5	·464
33°½	1· 5	·537	76°	·25	·269
38½°	1· 25	·566	90°	·0	·0
45°	1	·578			

NOTE II.—*On the Coefficient of Friction of the Front of a Screw Blade.*

The motion of water in two dimensions past a plate set obliquely in the current, has been investigated by Lord Rayleigh, by aid of a method invented by Kirchhoff. The solution involves infinite surfaces of separation, enclosing an infinite mass of dead water in the rear of the plate, and this is the only way in which motion can take place without loss of head. In fact, however, the surfaces of separation are finite, and enclose eddying water at the rear of the plate, in consequence of which, loss of head takes place. Yet there is reason to believe that in these cases, the stream line motion in front of the body does not differ much from the ideal case, and hence Lord Rayleigh's results may probably be applied without serious error.

Now this solution shows that the water passing a plate A B divides into two streams, one of which passes on the side A, and the other on the side B; the point K on the plate where the streams, divide being given by the equations,

$$\mathrm{AK} = \frac{2 + 4 \,.\, \cos\phi - 2\cos^3\phi + (\pi - \phi)\sin\phi}{4 + \pi \,.\, \sin\phi} \,.\, \mathrm{AB},$$

$$\mathrm{BK} = \frac{2 - 4 \,.\, \cos\phi + 2 \,.\, \cos^3\phi + \phi \,.\, \sin\phi}{4 + \pi \,.\, \sin\phi} \,.\, \mathrm{AB},$$

where ϕ is the (acute) angle which the plate makes with the direction of the current.

At K the water is at rest, while at points between A and K, or B and K, it moves towards A or B respectively, with a variable velocity which we call v. The friction on the plate is the difference between the friction on A K, and the friction on B K: for example, in the extreme case where the water strikes the plate perpendicularly, the friction is zero.

Thus if f be the ordinary coefficient of friction for a surface F, the total friction,

$$\mathrm{F} = f \,.\, \int_0^{\mathrm{AK}} v^2 \,.\, dx - f \,.\, \int_0^{\mathrm{BK}} v^2 \,.\, dx,$$

dx being an element of length of the plate.

Evaluating the integrals by use of Lord Rayleigh's results, we find

$$F = f \,.\, V^2 \,.\, \frac{4 \,.\, \cos\phi - (\pi - 2\phi)\sin\phi}{4 + \pi \,.\, \sin\phi} \,.\, AB,$$

where V is the velocity of the stream, and therefore the value of the virtual coefficient of friction f' is

$$f' = f \,.\, \frac{4 \,.\, \cos\phi - (\pi - 2\phi)\sin\phi}{4 + \pi \,.\, \sin\phi} = kf.$$

The numerical value of k is found to be for

$\phi = 0,$	$10°,$	$20°,$	$30°,$	$90°,$
$k = 1,$	·760,	·576,	·434,	0,

showing a rapid diminution as ϕ increases.

Mr. Froude has pointed out that the friction at the back of the blade likewise diminishes when the angle exceeds a certain limit, and states that his experiments show a great diminution in the friction as the angle increases.

DISCUSSION.

Mr. W. H. White said: Mr. Cotterill has given perhaps more attention to mathematical investigation of the theory of screw propellers and their efficiency than almost anyone except Mr. Froude and Professor Rankine; and in this paper, as I understand it, he takes the two investigations which are due to Professor Rankine and Mr. Froude, and shows them to be extreme cases; and he attempts an investigation which really connects these two methods, and shows how they are related to one another. I have been looking for Mr. Cotterill's paper with the greatest interest, and I am sure that I only express the feeling of many gentlemen present in saying that it is a paper to be read quietly and studied in comparison with those papers of Professor Rankine and Mr. Froude to which Professor Cotterill has alluded. I am sure that Professor Cotterill would not put forward anything here which does not deserve the most careful attention we can give it, and I for one shall take the earliest opportunity of making a study of the paper.

Mr. J. MacFarlane Gray: Mr. White has expressed exactly my opinion in this paper. This paper is not in a suitable form for reading here. I think there really ought to be some arrangement to make our meetings more useful, because the time has been almost wasted with regard to most of us in listening to this, no doubt, excellent paper. Those who know something of the subject to be laid on the table should have sent to them before the meeting takes place, some outline of what the paper is to be, as they do in the Civil Engineers' Institution, and then the discussion would be a great deal more valuable. I hope that the remarks I am making may have some influence with the Council, and induce them to make some arrangements of the sort which I have mentioned, so that we may know something as to what the paper is to be about.

Mr. J. Wright: I merely wish to remark that I think it would not be doing justice to the valuable matter in the paper Professor Cotterill has given us if we were to attempt to discuss it now. As Mr. White says, it is a paper which requires to be studied. If these papers could be put in the hands of members some time before the meeting, as Mr. Macfarlane Gray has said, it would no doubt

be a great advantage. I think it is not altogether the fault of the Council that this is not done because if the papers were sent in sooner they could be printed and distributed before the meeting takes place. Perhaps arrangements can be made in future years to admit of this being carried out.

Mr. J. Scott Russell: My Lord, I quite agree that this is a valuable paper, and I quite agree on the impossibility of giving it adequate discussion on so short a notice. Permit me to observe that a great attempt was made this year by the Council and the executive to get in the papers as early as possible. The difficulties in the way of that are serious, because the members have difficult calculations to make, and do not like to send them in until they are completed; but I think the advantages we have had this year, much more than in any other years, of getting these papers printed now, and the prospect we have of getting our volume immediately, are so great, that I think we do well to urge on the Council and the executive to take every means of getting the papers printed, if possible, and put into our hands some time before the meeting, because it is quite evident that we come here to give each other the benefit of our opinions upon those particular subjects, and we do not like to communicate opinions which are rashly formed, on a cursory investigation of the paper, and therefore it would be an enormous benefit if we could get them earlier. I mention it here now, my Lord, so emphatically because it is not the Council alone, or the executive alone, that will enable us to do it next year. It is the members present who are going to read papers next year, who, if they would be kind enough to send them in very early, would confer on us a great benefit. Now, permit me to say on this point that I am myself one of the greatest sinners, and I make that confession with all due humility; but I join in agreeing that the evil is one that is very great, of gentlemen keeping back their wonderful ideas and their elegantly new language until the last moment. I beg to say that I think the paper of Professor Cotterill most valuable, and I do not talk on the subject, because I think nearly everything has been already said that is worth saying.

The President: I think I may now ask Mr. Cotterill to address himself to the observations that have been made, and I am very sorry myself, with many others, and Mr. Scott Russell in particular, that the discussion of this important paper should have been curtailed through the unfortunate fact that those who are good enough to send papers here for discussion, do not always send them in good time. It has been suggested now in the course of this conversation that the Council should address itself to this question. The Council has done so, and in the course of the present session—only yesterday morning—we had a good deal of discussion on this subject, and considered whether or not there was any manner in which we could enforce more strict observances of our regulations, but it was considered to be a very difficult question. There is one law, no doubt, that we could make, and it would be a very effective one, which is, that any paper not sent in before a given day should not be read at a meeting. That, I have no doubt, would be a very conclusive sort of regulation, but the possible tendency of it might be this, that when the day of our meeting came we should have no papers at all. I am afraid that our friend Mr. Scott Russell has been known to be an offender in this case. In fact, he has said so himself, and while he is preaching to others, he himself has been a castaway. I think it would be well for him to bear that in mind. I am glad that the discussion has arisen because everybody who reflects upon it must see that it really bears upon the utility of our proceedings here. It is a great object, one of our greatest objects, to have good discussions on the papers read, and you cannot expect to have good discussions on these difficult and scientific papers unless the members of the Institution have them to look at beforehand. Above all, I must thank our friend Mr. Scott Russell for the candour with which he has avowed his own faults, and I hope I may add for the frankness with which, like other naughty boys, he has said he will never do so again.

Mr. J. Cotterill: My Lord, I can only plead guilty to the charge of sending my paper in too late, and, like a naughty boy who has seen the error of his ways, promise never to do so again.

ON TWIN SHIP PROPULSION.

By G. C. Mackrow, Member.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

In the year 1873, when the Channel Steamship Company applied to the Thames Ironworks and Shipbuilding Company, to construct a steamer on Captain Dicey's twin-ship principle, we endeavoured to obtain some particulars of previous vessels built upon this plan, but nothing could be obtained but a most meagre account of the *Gemini*, a wooden vessel which had been built some thirty years ago, and had plied for a short time upon the Thames. Others had been built in Scotland, but no reliable data could be obtained as to power, speed, &c.; consequently the results to be realised in the new ship were always felt by most concerned in her construction to be of a doubtful nature.

The vessel, as you are no doubt aware, has been built, and I have thought the particulars of so novel a vessel, with a few remarks upon some phenomena connected with the movements of the particles of water about her hull, should have a place in the records of this Institution; and the more so when I bear in mind the fact, that no system of construction has perhaps been the subject of so much criticism, both in the daily papers and scientific journals. It is needless to dwell upon the object Captain Dicey had in view in proposing this type of vessel, viz., for the purpose of reducing the horrors of the middle passage to a minimum, but let it suffice to say that, having been a captain in the Indian Navy, he had constant opportunities of witnessing the behaviour of the flying proas or sailing craft of the Indian Seas, as well as the double canoes of the South Pacific; which craft, in order to their carrying a large amount of sail, have in the first case, a pointed baulk of timber held at some distance from the vessel, by means of two spars, to act as a counterpoise when the wind tends to heel the vessel, as seen in the model on the table. In the second case, the natives build two complete canoes and connect them together, and these craft skim along the water at a great speed, and heel but little through the influence of the wind. (See illustration on next page.)

From this, Captain Dicey conceived the idea of two vessels secured side by side, the one acting as a counterpoise to the other, each in its turn preventing its fellow from rolling. The question then arose as to the desirability or not of constructing two complete hulls, or two halves of one hull, and it was eventually decided to build two halves, as in the model on the table, these half hulls to be connected by a system of cross girders, the four engine and boiler-

room bulkheads forming the main transverse-girders which span the channel between the two hulls, and enter into the construction of the hulls from the keels to the upper decks, and the collision bulkheads at each end, forming similar connections, the spaces between the bulkheads or transverse girders affording the passenger cabin accommodation. The *Castalia*, named after Lady Granville, was eventually thus built, and her plans and model I have the pleasure to submit to you. (See Plates XII., XIII., XIV.)

The *Calais Douvres*, a second vessel, has since been constructed with two complete hulls, without any advantage, it would appear, being derived from this system as regards speed, her

co-efficient of performance being much lower than that of the *Castalia*, but having the disadvantage of throwing the extremities of the hulls some 18 ft. further apart, thereby asking for additional material to make the end girders of equal strength, or with the same material to have less strength.

Dimensions of "Castalia."	ft.	in.
Length, extreme	290	0
Breadth of each hull	17	0
Distance between the hulls	26	0
Draught of water as designed	6	6
Area of section of both hulls	209·5	0

Superstructure 168 ft. long, 60 ft. broad, 8 ft. high, affording a magnificent promenade at a height of 14 ft. above the water line.

The power of the engines as originally designed was 1,250 indicated horses, which it was hoped by the promoters would have given a speed of 14 knots, but of which I always felt doubtful, knowing that in such a type of vessel there must needs be a larger increase of surface friction and other retarding influences not found in ordinary vessels. I was not in any way responsible for the speed or power of the engines, as the contract for them had been previously signed by Messrs. J. & A. Blyth; the Thames Ironworks being contractors for the construction of the hull only. There are two pair of diagonal direct-acting engines, one pair in each hull or half hull, with cylinders 46½ in. diameter, and having a stroke of 4 ft. 6 in.

The paddle-wheels were common, 22 ft. diameter over the floats, and the floats 20 in number, 10 ft. long and 2 ft. 3 in. wide, with a dip of 3 ft. 8 in.

The boilers, as originally designed, were 6 ft. in diameter and 14 ft. long, common, straight-away boilers, with one furnace in each, and eight boilers in all, namely four pairs. This arrangement was subsequently altered, at the suggestion of the Company's consulting engineer, to four boilers, 7 ft. diameter and 6 ft. long, with return tubes, and three furnaces in each. With this arrangement probably not more than 9 knots speed was obtained, as the boilers primed so badly that the engines could not be worked up to their full power, though many eminent men, the late Mr. John Penn among the number, did their best to obviate the priming, and eventually a contract was made by the Channel Steamship Company with Messrs. Maudslay, Son & Field for new boilers, and paddle-wheels with patent floats; these boilers, four in number, were 12 ft. diameter, 10 ft. 2 in. long, and the new wheels were 21 ft. diameter over floats, which were 9 ft. long and 3 ft. 6 in. wide, with 4 ft. 3 in. dip. This extra weight increased her draft to 7 ft., and the results of the measured mile trials gave a mean speed of 10·955 knots, with an indicated power of 1,516 horses, and a total area of midship section of 232 square feet, giving as her coefficient of performance 200 on midship section and 100 on displacement, the lowest I had ever noted; and it is to seek for some explanation of the cause of this excessive waste of power that I invite the discussion of the members of this Institution.

Mr. Froude, in his valuable paper, read here in the Session of 1877, on the effect produced on the wave-making resistance of ships by length of middle body, has thrown some light upon the subject, and as the *Castalia* had of necessity a long parallel middle body, in order to provide sufficient displacement, her bow-wave dies away before the run is reached, and so the benefit is lost of its power of propulsion. Up to the time of Mr. Froude's experiments being made public, it had been generally accepted as a theory, that once a channel had been formed to the extent of the largest area of section, the power needed to propel a vessel with a given length of parallel middle body, was comparatively small, amounting to merely that requisite to overcome the increased skin-friction. There can, however, be no question that the bow-waves when formed, falling in under the run of the vessel, very materially assist in her propulsion. There was, however, a very unlooked-for phenomenon noticeable the first day the *Castalia* steamed in the Victoria Docks, and to which I must draw your attention, as showing how mistaken are our thoughts oftentimes of what we have not actually proved. It was stated, I believe, by Mr. Reed and Mr. Merrifield, when the subject of twin ships was under discussion,

that they believed the water would be carried away by the wheels from the channel between the two hulls faster than it would flow in, and consequently there would be a hollow formed just before the wheels; evidently assuming that the water would flow in at one end of the channel as it was driven out at the other, and this no doubt was a very natural assumption, but one that proved to be wholly fallacious, as I propose to show.

Upon the day of the *Castalia* first steaming in the Victoria Docks, her bows were placed against a jetty on the north shore, and the wheels set in motion, when a very strong current of water was sent across the whole breadth of the docks, a distance of some 400 yards, setting the vessels rolling over on the south shore; and such was the strength of the current formed that the dock tug was unable to tow a vessel across it, and the *Castalia* engines had to be stopped in consequence. Now, anyone witnessing the sight of this stream of white foam, flying like a millstream from the after part of the wheels across the docks, would very naturally expect to find a current of water flowing in at the other end and eddying round the stems from the outside of the vessel; to my astonishment, however, when I looked down between the hulls at the fore part, I found chips of wood floating perfectly motionless on the surface of the water, and I called Captain Dicey's attention to it. We at first thought the engines must have stopped; we ran aft, however, and found they were still working, but we had no means of seeing that day what was taking place nearer the wheels. Before the next day of steaming we got a boat between the hulls, and Captain Dicey, myself, and the chief officer, with a few others got into her, not without some fears having been expressed on the part of some that the head-fast of the boat was not strong enough to hold us against the current which it was believed we should find as we neared the wheels; and certainly as we neared them, we appeared to be approaching a sort of miniature Niagara Falls.

The sequel proved, however, that there was not the slightest ground for fear, for while the engines were going full speed, and the water was driven at the rate say of 20 ft. per second from the after part of the wheel, on the fore side up to the very point at which it was struck by the float, the water was all but motionless; a piece of batten was floated up to within an inch of the float and returned to us, was again thrust towards the wheel, was struck by the float, and instantly disappeared. Now that the facts are stated, I have no doubt the members of this Institution will at once see that this is what we ought to have expected, for as the paddle-wheels scoop out the water a depth varying from 0 at the surface to 3 ft. 8 in. at the extreme dip of the float, so the particles of water from below thrust themselves upwards at a velocity due to the pressures at the various depths, say from 0 lb. at the surface to 228 lbs. per square foot at the lowest depth. It may, however, occur to the minds of some, that these features of the case are materially altered when the vessel is under weigh, as in that case the wheels are advancing into the body of water before them; but granting this to be the case the vessel does not advance at the same rate that the wheels remove the water, as their revolutions, say 20 per minute, give a speed of 23 ft. per second, whereas the vessel would be advancing only at the rate of 18 ft. per second. The deductions I believe, then, we may draw from these facts are, that the particles of water that rush to supply those displaced by the revolutions of the wheels, come across under the hulls of the vessel from the

outside in a partly athwart-ship direction, and thus create a considerable augmentation of resistance.

It may perhaps be thought by some that this is not a correct assumption. It appears to me, however, that seeing the water does not flow in from before, and that it cannot clearly flow in from behind, there remains but the sides as the only way of approach, the water sinking down from the surface to fill up the vacant space. Another phenomenon noticed upon a later occasion, namely, some sawdust thrown down into the wheels, was found coming up from under the bottom on the outside of the hulls, clearly showing that the water, being confined between the hulls and compressed by the action of the descending floats, was carried down to the bottom of the hulls, where it was allowed to expand, and so was eventually carried across to the outer sides of the hulls, and then arose to the surface of the water, thus giving us a further clue to the enormously augmented resistance in this class of vessel. Were the wheels placed on the outside these two objectionable features would doubtless disappear.

This paper would not, however, be complete without a few words being said upon the steering qualities, as it was thought by many competent to judge of such matters, that she would not be found to answer her helm very satisfactorily. It was with some anxiety, therefore, that we at first cast off the Victoria Dock tug in that crowded part of the river opposite the dock entrance, and put her under her own helm. A few minutes were sufficient to show us that she was wonderfully under control. I noticed however, later on, that she did not appear to pay any attention to her helm, even though in motion, until the stream of water from the wheels had reached the rudder, when the effect was most marked. That is to say, I have frequently noticed that when she had some way on her, the engines being stopped, that she paid no regard to her helm; but as soon as the engines were started, and the water rushed past the rudder, she obeyed her helm instantly; in fact, she has been so perfectly under control at all times that she is put alongside the Piers at Dover and Calais with perfect ease. I show a plan of the rudders (See Figs. 4, 5, 6, 7, 8, 9, Plate XIV.), to which I gave particular attention, so many misgivings upon the subject of her steering having been expressed. They work, yoked together upon the Rapston slide principle, by means of a Brotherhood's engine and endless chain, one man being ample to work both rudders. Hand-steering gear is also provided, in view of a possible failure of the engines.

I conclude by adding my own experience of her behaviour embodied in a letter I wrote to the *Times* in October, 1875, which I need not trespass on your time to read, but which I append with an outline specification giving her main scantlings, which will be found to be comparatively light, but have proved to have been amply sufficient. I should, however, not recommend any reduction in the scantlings for a similar vessel, unless built of steel and good material, and careful workmanship must of necessity be employed.

I append also a few particulars of the *Calais Douvres* culled from the public papers, and regret I cannot give more, though application was made to her builders through Captain Dicey. I have no doubt, however, that as far as they go they are correct.

Dimensions of "Calais Douvres."	ft.	in.
Length, extreme	300	0
Breadth ,, ...	62	0
Breadth of each hull....	18	3
Distance between hulls	25	6
Draft of water	6	7½
Area of sections	230 sq. ft.	

Engines, Diagonal, direct-acting, with—
Cylinders 63 in. diameter, 6 ft. stroke.
Wheels, 24 ft. diameter, over floats.
Floats (feathering), 10 in number, 10 ft. 6 in. by 4 ft. 9 in.
Boilers, 4 in number, 15 ft. diameter, 19 ft. long, fired from each end through 3 furnaces.
Indicated horse-power, on trial, 4,270.
Speed (mean), 14 knots.
Coefficient on section, 148.

APPENDIX A.

Extract of a Letter to the "Times," *October 5th*, 1875.

Until Tuesday last I had not been fortunate enough to be on board during any weather that would enable me to form a judgment of her behaviour in a heavy sea. Leaving Dover, however, on the morning of that day, we encountered a strong gale from the south-west, which being "abeam" tested her rolling qualities very fairly, if the term "rolling" can be applied to her movements. For, strictly speaking, she does not roll, but as each wave passes, the windward hull lifts to it and falls again, the wave passing across the space between the hulls to lift in turn the leeward hull, but not to set up a roll, for if it were possible to stop the next wave from passing she would remain motionless, whereas in any single-hulled vessel the oscillation set up would continue for some length of time, and she would gradually come to a state of rest, and it is the above peculiarity that gives its chief value to this form of vessel. These movements of necessity cause an alteration in the horizontal deck platform, but not more than three and a half degrees on the above occasion, and it was the opinion of her able commander, Captain Pittock, that no weather she was likely to encounter on this station would give more than five degrees of inclination, the present steamers rolling fourteen to fifteen degrees. With regard to pitching and scending, I may add that while at Calais on the above-mentioned day the wind veered round to the north-west, and so on the return passage we had a strong sea ahead, into which the mail steamer was pitching, taking in the water through her hawser pipes and over her bows, sending it forth in white sheets over her waterways, while on the *Castalia's* low deck forward might be seen at work an engineer, repairing a damaged float nearly the whole of her passage, her oscillation in a fore and aft line not exceeding two and a half degrees.

From the above remarks it is evident that so far as form is concerned, Captain Dicey has hit upon the right type of vessel for reducing the horrors of the middle passage to a minimum. It may, however, be fresh in the memory of your readers that various objections were raised some three years ago, when first this proposition was brought before the public; they were summed up in a letter to you, if I mistake not, by Mr. E. J. Reed, M.P., and reduced to five in number, which were as follows. First, plan objectionable, as involving too great draught of water. Second, difficulty in con-

necting two hulls together. Third and fourth, two vessels yoked together unfavourable for high speed. And fifth, the unusual breadth unadapted to the harbour of Calais. With regard to the first objection I have only to add that the *Castalia* complete with all stores, coal, &c., on board, is drawing 7 feet of water, about the same as the mail boat on that station. With regard to the second, I need scarcely say that this part of the design received our first and most careful consideration, and was further submitted to that thoroughly practical body, Messrs. Lloyd's, receiving their fullest investigation, and special survey of the vessel while building, by their most eminent officers, and from all that we can discover from the time of launching (a severe test in itself) to the present time, after a most careful inspection during and after the gales of Monday and Tuesday last, not the slightest trace of want of strength is discernible, not a single rivet or bolt of deck, or waterways can be found, showing signs of straining; and this, coupled with the total absence of all vibration so common to steamers, may fully justify the conclusion that the second objection has been met. With regard to the third and fourth however, we are obliged to yield, her coefficient of performance being a very low one, as the same power applied to an ordinary vessel would have propelled her at a speed of fourteen knots. The placing of the propellers in a projected vessel is consequently giving us some difficulty, as the limited draught precludes the adoption of screws, and the extra breadth involved in placing the wheels outside is objectionable for several reasons, but not so much on the ground of any difficulty in entering Calais Harbour, the last of the series of objections raised by Mr. Reed, as the remarkable ease with which she answers her helm, coupled with the great lateral resistance she possesses, proves of the greatest advantage to her upon such occasions. It had, however, been felt by many naval officers, Lord Clarence Paget among the number, that she would be severely tested when strong easterly or westerly gales prevailed; her great height out of the water presenting so large an area to their influence, it was feared after having partly entered the harbour she might be driven to leeward, and thus thrown across the entrance. Of this, however, from the above qualities being possessed by her, it is now confidently felt there is not the slightest danger.

APPENDIX B.

Outline Specification.

The hull to be constructed on the longitudinal and transverse system, the plates to be capable of bearing a tensile strain of twenty-one tons lengthways, and eighteen tons crossways. Each hull to be divided into watertight compartments by means of eight bulkheads in each, the four central ones to run up to the height of saloon deck, and to be carried across the whole breadth of the superstructure and supported by partial bulkheads, thus forming a system of transverse girders.

Stems.—Of wrought iron $6 \times 2\frac{1}{2}$ in.

Frames.—Of angle iron $4\frac{1}{2} \times 3 \times \frac{6}{16} = 2$ ft. apart.

Reverse Frames.—Of angle iron $2\frac{1}{2} \times 2\frac{1}{2} \times \frac{5}{16}$ in.

Longitudinal Frames.—Of plate iron 24 in. deep and $\frac{5}{16}$ in. thick. Angle irons on upper and lower edges, $2\frac{1}{2} \times 2\frac{1}{2} \times \frac{5}{16}$ in.

Floors.—Of plate $\frac{5}{16}$ in. thick and 26 in. deep.

Beams.—Of angle iron $6 \times 4 \times \frac{3}{8}$ in. on alternate frames.

Stringers.—Of plate iron 36 in. wide, $\frac{3}{8}$ in. thick. Angle iron $3 \times 3 \times \frac{3}{8}$ in. Lower deck, ditto of angle iron $3 \times 2\frac{1}{2} \times \frac{5}{16}$ in. and plate $\frac{3}{8}$ thick.

Deck Ties.—On main deck 30 in. wide and $\frac{5}{16}$ in. thick.

Iron Deck.—On top of superstructure $\frac{5}{16}$ thick, worked, jump jointed with edge and butt strips.

Bulkheads.—Placed where shown on plans, of plate $\frac{5}{16}$ in. thick, supported by angle irons $2\frac{1}{2} \times 2\frac{1}{2} \times \frac{5}{16}$ in. placed 2 ft. 6 in. apart.

Hull Plating.—The strakes under first longitudinal $\frac{7}{16}$ in. thick, the other strakes up to main deck $\frac{6}{16}$ in., as also the upper strake of superstructure; the rest $\frac{5}{16}$ in.

Girder for Plummer Blocks.—To be formed of plate and angle irons as per sketch, and to be sufficiently strong to carry the two outer ends of paddle shaft.

Wood Decks.—Of pine $6 \times 3\frac{1}{2}$ in.

Ceiling of Hold.—Of elm $2\frac{1}{2}$ in. thick. Room and space battens of pitch pine $1\frac{1}{2}$ in. thick above this.

Boats.—Six in number, viz., four 25 ft. cutter life boats, one 14 ft. dingy, and one 20 ft. cutter.

Anchors and Chains.—Four bower anchors, Martin's patent, $15\frac{1}{2}$ cwt. each, and 300 fathoms of $1\frac{5}{16}$ in. chain cable.

Life Rafts.—Two in number of plate iron, formed of a pair of rectangular tubes lashed a few feet apart, and two others on Captain Dicey's plan of wood and cork.

Sundries.—Awnings, &c. &c.

DISCUSSION.

Mr. W. John: My Lord, I do not think that a paper of this importance should pass without some one offering a remark upon it, and I rise the more readily because Mr. Mackrow has alluded to the Society to which I have the honour to belong as having approved the construction of this vessel. Mr. Mackrow will remember when the drawings first came to us we discussed the matter with him, and there is one essential element in the construction of this ship which it would be well to point out to the meeting. I think it was a point we rather strongly pressed upon Mr. Mackrow's notice. It is as to the connection of the two sides of this model. I may say the weight of the hull of this vessel is kept down very low indeed, and it was a question of very great interest to combine those two hulls without adding considerably to the weight. I think I can explain the principle on which it was eventually done in this way: owing to the wheel coming in the centre of the vessel you see the two vessels are separate and you cannot form a connection there, but by means of these two engine-room bulkheads put right across the vessel, before and abaft, you have a means for forming a connection—you have, in fact, two sides of what can easily be made tubular girders as it were, and by plating over between the two bulkheads *here* and the two bulkheads *there*, in an athwart-ship direction, and with the roof of the tunnel, forming the lower part of the girder, you connect the two hulls by two large tubular bridges, two perfect tubes, which assist and hold them both obliquely and transversely, and for that reason you will find in the design of this vessel there is a partial iron deck up *here* (pointing to the model), but it is a partial iron deck put for purposes exactly the opposite of those for which the iron deck is usually put. It is not for longitudinal strength essentially, but for transverse strength, and there are, as it were, strips of iron deck going across the ship. It is by means of that system of having two absolute trunks from ship to ship, bridging over the space between, that you get the absolute freedom from movement which there is in that ship. I only rise to make that clear, because it is a point of very considerable technical interest in connection with a vessel of this light construction having the two hulls connected together, and yet, after working as it has done for some time, there is no sign of movement. The same principle, I may mention, has been carried out in pretty much the same way in the *Calais-Douvres*, which, with still more powerful engines, going at a higher speed, still keeps that rigidity, and shows there is combined with lightness, and a very peculiar-shaped ship, and very trying circumstances, a sufficient amount of rigidity.

Mr. Henry Liggins: My Lord, I listened with very great satisfaction to the expression of the hope which is generally felt, I believe, in this Institution, that the mercantile service ships should receive more attention than they do, and the scientific features in connection with those ships should be considered as well as those of Her Majesty's Royal Navy. If that is a necessity, which I believe we are all agreed upon, it is very desirable indeed that shipowners should consider it in a pecuniary point of view; and it is also important that the difficulties which attend the Channel service for the transit of passengers should also receive the consideration of this scientific body, because the public at large do hope that out of the science of this Institution some suggestion may be made of a practical nature to get over the difficulties of the Channel passage between England and the continent of Europe. The subject under discussion has been brought before this Institution during the last ten years by the most eminent of its members, notably Mr. Scott Russell, who claims the right to speak with very great authority, having constructed the largest ship the world has yet produced. Mr. Reed has also addressed you upon it. The late Mr. Grantham also brought before you a very able paper, and I quite remember some years ago making a humble effort to contribute to the literature on the subject by reading a paper. I think I can state, without fear of contradiction, that I am the only person belonging to this Institution that ever made a passage in the *Gemini*, the vessel having proved a complete failure. And there I will correct Mr. Mackrow, if he will allow me, and ask him to strike out the word "ply" from his paper in connection with the *Gemini*, because she never arrived at that stage; she never went but once down the Thames, and as far as I was concerned she only brought me half-way back again. She belonged to a friend of mine, who built her believing, from what he had heard, that a vessel with a widely spread-out deck would be a favourite with the public for pleasure traffic; he believed that with paddles placed between two hulls he could get high speed and large deck area. He spent £14,000 on this little vessel in the hope that he would be able to eclipse the large fast steamboats of that day employed between London and Gravesend. We struggled down the river Thames and got to Gravesend; we remained there an hour, and we endeavoured to struggle back again, but only got to Erith, when, by universal consent, the visitors were glad to leave and get back by the next up Gravesend boat. She was a complete failure; she never made but that one passage, and never plied for hire. Now we come to the next ship of similar type, the *Castalia*, which is before you, and I do not think Mr. Mackrow cares to claim much credit for the design of the ship. He was asked by Captain Dicey to build a ship with two hulls, and he thought out carefully how to carry out his instructions with enduring strength of structural form, and used much energy coupled with his scientific knowledge as a Naval Architect to produce the best result practicable under the many difficulties of this novel form of vessel. Lloyd's were consulted, because, no doubt, a great amount of dread was felt with regard to that form of construction, as to whether or not, in a heavy sea in the channel, the hulls might not separate from each other; therefore Lloyd's were consulted, with the view of getting an opinion on the strength of the girders that might be deemed necessary to keep the two structures rigidly together. The ship went to sea, I think Mr. Mackrow said, in 1875, and she did ply that summer between Dover and Calais, with a considerable amount of success as regards the satisfaction which, for a time, she gave to the public, but as soon as bad weather set in she was withdrawn, and from that time to this I believe the vessel has been lying in the Thames just below Woolwich. There were reasons why it was considered by her owners desirable to construct a ship of the same type, but of a better form. They asked for tenders, and a vessel was built called the *Calais-Douvres*, but that vessel is in no respect superior to this, and I think the two hulls of which she is made, there being two half hulls, is not an advantage which anyone is able to appreciate. I have taken a good deal of pains to watch the working of the *Calais-Douvres*. It was a long time before I got a chance of seeing the *Calais-Douvres* in anything but smooth water, and a smooth-water passage across the Channel from Dover to Calais is not one that any other boat cannot

do with satisfaction to the passengers; but in September last I was able to catch the *Calais-Douvres* on two days when it blew rather hard. I was on the Admiralty Pier on one occasion when there was a good deal of cross sea, the wind was about W.S.W., and the tide was ebbing, which many gentlemen here know would produce very rough water indeed in the neighbourhood of Dover. After waiting some time the Ostend boat arrived, one hour and forty minutes before the *Calais-Douvres* made her appearance, and the trains were kept waiting one hour and forty minutes for the arrival of the *Calais-Douvres*. She came through the mist which enveloped her and showed herself, and I watched her with the keenest interest. She rolled, or she moved—Mr. Mackrow does not like the word rolled in connection with her, but it appeared to me that she did roll—not an honest roll, but I may call it a wobble, which if not the ordinary roll of a Channel steamer, was quite as objectionable to her passengers. She approached nearer and nearer, and with the aid of a powerful glass I watched her keenly, and she was coming straight end on to where I was standing on the Admiralty Pier, and the part between the two hulls seemed to me to be completely choked with water, which sufficiently accounted to my mind for the very slow progress she was making in comparatively smooth water for so large a ship, she being 300 ft. long. You would not have expected so much motion in an ordinary Channel sea with a ship of that length, but she seemed to me to be completely choked between the two hulls, and that was enough to prevent her making rapid progress. She took green seas all over that lower deck. In fact, I very much doubt if it would have been safe to have stood on that day on that lower deck. Not having been on board, I am only speaking of what appeared to me to be the case from the Admiralty Pier. She came in, and I must say that she steered very well indeed. I was told that there were 460 passengers on board, and I should think from the observation which I had the means of making that that was about the number, but I do not hesitate most solemnly to declare that at least 400 of those were sea-sick, for I do not think in my forty years' experience in the English Channel, or in my forty years' experience of crossing the Atlantic, I ever saw so many people sea-sick in one day as there were there. My son was with me, and we both counted the people as well as we could, and we afterwards noticed that a man and a boy were employed on the deck of that ship for half an hour and upwards in washing up the basins which had been used. That is very clear proof that those travellers that day had not derived any benefit from a slow large ship. There was not a particle of her deck that was not wet with the spray blowing over, and she is not protected as a comfortable vessel ought to be from the wind or the spray, because there is nothing but wirework all round, just to prevent you tumbling overboard; there is no protection for ladies, whose dresses are apt to be blown up on the deck of a ship if they are not screened by high bulwarks. The ordinary Channel steamers have very high bulwarks, and they are a great protection against the weather, and they put up weather-cloths fore and aft which keep off the wind and the spray, but there was nothing of the kind in the *Calais-Douvres*. I went below to examine every part, to see her under circumstances of a roughish passage, although I had seen her before. The cabin windows had all been closed, and the heat and offensive smell in this part of the vessel made it utterly impracticable for anyone to remain there. It was impossible from the heat of the boilers that anybody could have remained for a quarter of an hour in those cabins without being almost parboiled. I assure you that I do not exaggerate in any word that I use, the heat was most excessive, and at this moment, if you go on board the *Calais-Douvres* you will find that the partitions and the bulkheads, or many of them, are cracked, and all of them are blistered from the excessive heat from the boilers. They are close to the boilers in the lower part of the vessel. That is an objection on rainy days, when there is no suitable place to go below and lie down. That is the *Calais-Douvres* as she was last autumn. Mr. Mackrow says that she steers well, and I say that she did steer well that day. She steered very carefully, and came to an inch alongside the wharf, but I can tell you this, that they dare not take her on rough days into the harbour of Dover; and on

that occasion she had to go to the Downs to lay up for the night, and came back the first thing in the morning to take up her position. She does not go into Dover Harbour to rest as the ordinary boats do, and therefore I suspect they are afraid of her not steering with the accuracy which is necessary for the very narrow harbour of Dover. It is known to be an awkward place, and the tide is very strong at the harbour's mouth occasionally in certain states of the tide. It is perfectly impossible that it can answer to take a ship to the Downs every night, to lay up there until the next morning, and that then she should have to come back that distance of seven or eight miles to commence her morning's work. It is harassing, to say the least of it, to the crew—who, by the by, were glad enough to get out of her. One of the men said, "Well, we are here to-day and gone to-morrow;" they cannot get the crew to stand by her on account of the many inconveniences which they suffer, and the labour which they have to go through. I do not know that there are any other points that I need touch upon. I may here observe that the system of the two hulls is a very ancient one. The services of a catamaran like that vessel under sail might be very valuable for the carrying of letters and despatches from ships through the surf at Madras, or through the surf on the beach of the South Sea Islands, where the water is very shallow, but it would be unsuitable, from the ballast it requires to render her stable enough to carry a press of sail, for any other purpose. It by no means follows that such a frail boat as that, which is most useful in its way, should be applicable to the carriage of passengers and merchandise in the English Channel. The catamaran is, really, for beaching in shallow waters, and only carrying a man or two with despatches to and from the ship. She is not intended to carry a cargo, nor is she capable of it. But it is a very ingenious unscientific method, because a savage invention of giving stability, but stability need not be placed in such form for the Channel service.

Mr. E. J. Harland: The particulars with which Mr. Mackrow has furnished us with reference to the results of trial trips, more particularly the *Castalia*, I think are very interesting, more especially not with regard perhaps to the probability of naval architects being able to reduce the principle of a twin-boat to practical use, but rather to indicate to us in what direction we, in such applications or attempts, may fairly look in the future for a loss of power. It appears to me that one of the principal reasons why a rather serious loss of power has arisen in these twin-boats may be traced to skin friction. It would almost point to this, that if a twin-boat could be closed together a little at the bow, and made a little wider at the other end, the probability is that that would be an advantage so far as going ahead is concerned, but as both ends are made alike in order that the vessel may be saved having to turn, it means that the ship would have to be reversed again in returning. That is with reference to the matter of skin friction. As to skin friction, a little circumstance occurred in my experience which may be interesting. Some years ago a gentleman—I think Mr. Fletcher, of Liverpool, an extremely able and scientific man—brought out a speed indicator, and this instrument was on the principle of running what he called a sword through the bottom of the ship, two, three, or four feet in length, and at the bottom of this sword there was a hole bored in the direction of the vessel, so that when the vessel went a-head the water was driven against the surface of this hole, and an indication was taken by a pressure-gauge in the vessel, and he could thus, by an ingenious mode of calculation which he proposed to adopt, indicate the different speeds of the vessel. You could read in the cabin, or in any other part of the ship, the speed at which the vessel was going; it was a continuous speed indicator. But it met with a very unexpected circumstance. This sword, which was easily passed through a stuffing box at the bottom of the ship, was first made 2 feet in length, and when it was found that the speed indicated was only about half what the speed really was, the length of the sword was increased to over 3 feet; but it was still found, even at 3 feet or 3 ft. 6 in. distance from the skin of the ship, 100 feet from the bow of a vessel something like 450 feet in length, that the indication was very far below what the ship was actually travelling, showing that there was a surface of water

following that ship at that part of the ship of fully 3 feet in thickness: it did not mean that to the extent of 3 feet that water was flowing at the rate of the ship, but it meant immediately next to the ship perhaps the water was travelling almost identically at her own speed, and every foot you left the ship so much the water lessened in speed with the ship, but even when you had gone to 3½ feet from the ship you had not arrived at a point where the water could be said to be undisturbed. That indicates to me very clearly when we are dealing with naval structures, particularly where, as in this case, we have an extraordinary amount of skin surface wetted, that we then have to prepare ourselves to meet with a very serious objection, an objection which perhaps may weigh, and in this case appears to outweigh, any of the other advantages for which we are anxiously seeking. With reference to the strengthening, to which Mr. John has referred, that struck me as most ingenious on the part of Lloyd's. Now with regard to the paddles, that is another illustration of skin resistance. Some little time ago we had occasion, in the construction of a paddle steamer, to go into an idea proposed by Mr. David Rowan, of Glasgow, a most ingenious and excellent professional gentleman. In most cases it has been customary to have the float of the paddle about 3 or 4 inches clear of the ship's side. We found there was a positive advantage gained by cutting off the float next the ship's side, so as to be well clear of the shell of the ship, so that although apparently we lost a considerable amount of float for the applying of the power, still we really with what remained gained a better result, and we got a stroke or two more out of the engines for propelling purposes than when the float had a greater surface and was nearer the ship. That, in my mind, also points in the same direction. It shows clearly to me that the skin surface friction was such that at least for a foot or two outside the beam of the ship the water was following the vessel, so that the paddle really was doing an extra amount of duty; and in fact was pulling against itself next the hull of the ship. Now with reference to the cross Channel trade and the cross Channel steamers, and the hitherto most unsatisfactory solution that has been offered, it is very unfair to naval architects, because they are blown up right and left for not putting something better on between England and France, when there is something so extremely satisfactory between Holyhead and Kingstown. To my mind it appears to be simply this. It is a matter of size of ship. You cannot have comfort at sea unless you have size, and in that size you must not forget the length. Now that means a large harbour, and sufficient water in that harbour. Unfortunately, whatever has been done at Dover, is only one side of the hedge. At the other side, in the French ports, not only of Calais but Boulogne, they are fearfully behind the day, and unless we first have satisfactory ports, the naval architect need not be asked or attempt to construct a satisfactory ship. Size is the only solution, in my opinion, of the difficulty. I do not think it will be in the direction of twin-boats. There are complications about their first cost, and other difficulties, which I fancy would result in our adhering rather to the old system of single hull. I am quite sure that there will be no satisfactory solution of the problem until the authorities on the French coast as well as on the English have settled it by giving us sufficiently large and commodious harbours, and when that is done the naval architect will very soon try his hand, and I have no doubt will succeed.

Mr. William Parker: My Lord, as I have taken a little interest in the propulsion of these two twin vessels, I have listened with pleasure to the remarks which have been made, and with your permission, I will make a few observations about them both. There is one thing that has been settled beyond a doubt by these two costly experiments, and that is, that naval architects or enthusiastic patentees will never again imagine that this form of vessel will be the ship of the future. You will appreciate this more readily when I mention the enormous power that is necessary to propel vessels of this form, and the amount of coal that has to be burnt to obtain that power. The *Calais-Douvres* will burn about 220 tons to propel her 14 knots an hour, and you will agree that it would be suicidal to attempt to burn anything like that in a merchant ship. Now, there is one thing in con-

nection with these two vessels, especially the *Castalia*, that strikes me, and that is, that before attempting to build the vessel some proper means were not taken to arrive at the power necessary to propel her 14 knots. It was very well known by the promoters of the Company, I presume, that unless this ship did steam 14 knots she would be utterly useless. Mr. Mackrow says he did not expect that 1,250 horse-power would give her that speed, but he said the promoters expected to get it. Now, I think a very great blunder has been made by some one in this respect, and I will ask Mr. Mackrow, in passing, who was responsible for the power, and who was responsible for that power giving the required speed? The other vessel, the *Calais-Douvres*, was built by Mr. Leslie, of Hebburn-on-the-Tyne, for the same Company, and intended to be run in the same trade. But what did he do when he commenced that ship? Having ascertained that it would be impracticable to run a vessel into those harbours over 300 ft. long (10 ft. longer than the *Castalia*), and that he could only be allowed 2 ft. extra beam and the same draught of water, he at once took measures to ascertain what power was necessary to propel such a vessel at the required speed. Two Woolwich steamers were chartered, they were taken to sea, lashed together, and a set of progressive speed trials were made with these two ships. He then disconnected them, and ran them singly, taking particulars of their performances. Having obtained a certain amount of data from this experiment, he took the *Castalia* out, and conducted a series of speed trials with her. From those two experiments Mr. Leslie had collected sufficient data to enable him to say, Neither 1,250 horse-power, nor 2,000 horse-power is sufficient to propel this form of ship; but I must construct engines so that they will be capable of exerting at least 3,600 horse-power. Mr. Leslie returned to the Tyne, he designed his ship, and he gave the engineers the power he would require to propel her 14 knots. The vessel was built, and the engines were constructed to develop that power, and when she was launched, with all her coal, and fittings, and everything on board, she drew about 6 ft. 10 in., being 2 in. less than the required draught; but the difficult problem Mr. Leslie had to solve was, having only 10 ft. more length, and 2 ft. more beam, how to get 250 tons more displacement in the same draught, which the extra weight of machinery required. He overcame that by an ingenious arrangement of the rudders. Instead of the vessel being very fine at both ends, he made her very full, and arranged the rudders so that they should be part and parcel of the vessel, being about 5 feet in width at the after ends, each rudder being about 10 ft. long. His vessel had about 20 feet more midship body than the *Castalia*. It was also to get this extra displacement that the elliptical sides were adopted.

Mr. J. Scott Russell, F.R.S.: Were both ends alike?

Mr. Parker: Both ends were alike, that was, 20 feet in length. As I said before, the vessel was built and launched, and she only drew that draught of water. She was tried at sea, and she indicated 4,100 horse-power, with a speed of a little over 14 knots; her bottom was very dirty, having lain in the water for sixteen months. After her bottom was cleaned she had a speed of about 14¾ knots on an indicated horse-power of about 4,300. Now, I have no hesitation in saying, having attended all the trials of both the *Castalia* and *Calais-Douvres*, that the *Castalia* is driven a great deal easier than the *Calais-Douvres*. But that was not the problem; the question was, can sufficient power be put into a vessel of these dimensions and this form to propel her 14 knots, and how can the extra displacement be obtained? This displacement could not be obtained without elliptical sides. Mr. Leslie had to produce a ship which burnt a great deal of coal, but still she did her work. The *Calais-Douvres* ran last season between Calais and Dover, and carried altogether about 51,000 people which was about 700 a day. She earned nearly £22,000, and she cleared 15 per cent. of her original cost, after all the expenses in connection with working her, including interest and depreciation, were paid. If that is not a success I do not know what is, so far as doing her work is concerned. I hear from the London, Chatham, and Dover Company that they are inundated with

letters as to when the vessel will be ready to run again. I think Mr. Leslie, who undertook this piece of work, deserves great praise and credit; because when he commenced this boat he contracted with the Channel Steamship Company, and two months after he commenced it the Channel Steamship Company failed. Mr. Leslie went on with this ship at his own cost, and with this prospect in the future, that if she did not attain the speed he would have to break her up; and if she did attain the speed, he would have to look out and get somebody to buy her. I think he deserves great credit for the bold manner in which he carried it out, and I think the London, Chatham, and Dover Railway Company deserve very great credit for at once taking it up, buying the boat, and giving the public a better means of transit between Dover and Calais.

Mr. J. R. Ravenhill: My Lord, I should not have troubled the meeting if it had not been for a remark or two which have dropped from Mr. Parker. The first question that I should like to put to him, so that I may feel quite sure that I am correct, is this: I understood him to say that the *Calais-Douvres* burnt 220 tons of coal a day.

Mr. Parker: Yes.

Mr. Ravenhill: I believe she only made one passage across the Channel and one back each day?

Mr. Parker: That was all.

Mr. Ravenhill: Then, taking her full time to be an hour and a half, on each passage 60 tons an hour would make 180 tons a day—40 tons for getting up steam and lying-to. That is an amount of coal which it appears incredible could have been supplied to the fires in the time named.

Mr. Parker: Twenty-four hours is considered to be a day at sea, Mr. Ravenhill.

Mr. Ravenhill: I beg your pardon. I was thinking of the Channel service, and when you said 220 tons a day I thought you meant 220 tons during the time in each day she was under steam or had her fires lighted. The thing puzzled me, and I am glad I have elicited that explanation.

Mr. J. Scott Russell: It is about 10 tons an hour.

Mr. Ravenhill: I think there is one thing about these twin steamers that have been placed in the Channel, and it is this: there can be no doubt that the horrors of that passage had been experienced by the travelling public over a long course of years, but I do not think you will see that class of ship multiplied. I am of opinion that if larger vessels were placed on that station built of steel, such as the *Maid of Kent*, and some of those built many years ago, when first the London, Chatham, and Dover Railway opened, or if those vessels were enlarged, you would obtain nearly all, if not quite all, the advantages to be derived from these twin steamers, and with very great saving as far as fuel is concerned at all events. Mr. Harland alluded to the great skin-friction. He did not put it quite before you in the point of view in which I should like to put it, which is this. In the case of the *Calais-Douvres* there is just twice the amount of skin-friction that there would be in the ordinary hull of a vessel of the same beam, and that in itself must be fatal to speed. I believe there are many naval architects who, if they were consulted, could give you a vessel built of steel at the present time, say length 300 ft., and breadth over all the same as the *Calais-Douvres;* but they would, instead of spreading the hulls, bring them together, and take the dimensions all over the ordinary old form of paddle-boxes. There might be the difficulty of turning that was alluded to by a gentleman who spoke some little time back, at all times on entering Dover Harbour; but if the public drive the railway companies into giving them accommodation across the Channel at the present time, when the accommodation in the harbours is of a very limited description, the companies must put up with the necessity of their vessels going into the Downs, or elsewhere, for the night to anchor. That is a matter that only affects the railway companies themselves. I do not think it at all affects the question as to the relative capabilities and performances of the ships. I believe that before very long we shall see improved vessels on the Dover station, but they will not take the form of the *Calais-Douvres.*

Captain Dicey: My Lord, I wish to make a few remarks with regard to what Mr. Liggins said about the Channel service. I have been here several times on previous occasions when this matter has been discussed, and I have always found Mr. Liggins throw cold water on every scheme that has been introduced. I should fancy Mr. Liggins, with the experience that he has had across the Channel, and to America, must be like one of those sailors on board the ordinary Channel steamers whom I happened to speak to some time ago, and said, "Cannot you get a better class of vessels to run across here?" "Oh," he said, "the passengers get used to it; they would not care about any improved steamer at all." With regard to the steamer going to the Downs, I watched the *Calais-Douvres* very frequently, and last year, during the time she was running, three months or three and a half, she went to the Downs only twice. That was in consequence of there not being water enough in Dover Harbour to enable her to go into the harbour, but every other time she anchored under the pier or went into the harbour. Captain Pittock said over and over again, "I can assure you, Captain Dicey, I would as soon navigate this vessel as one of our ordinary Channel-going steamers." As far as the difficulty of the ports is concerned the great difficulty is at Dover, not at Calais. You can always get in and under shelter at Calais, though you may have to wait an hour or two for the tide, but at Dover the harbour is dry at certain times, and therefore you are obliged to wait, and of course the steamer could not remain outside if it were blowing a gale of wind. With regard to the heavy passage Mr. Liggins has spoken of, I went down the day after the vessel crossed, and I asked the Captain and others aboard what kind of passage they had. They said, "A very rough passage;" and in fact it was so rough that the Channel steamer could not cross that day; and the Folkestone and Boulogne steamer could not cross that day; she was kept in Folkestone on account of the heavy weather, whilst this vessel crossed over with great comfort to her passengers—so much so, that they told me there had been very few sick. As a proof of that I spoke to the steward. I said, "Steward, you have no indicator on board this vessel; we have them on the *Castalia* to show what the incline would be in rolling." And he assured me that they never rolled, and though many of them had made a comfortable dinner on board, the ship had never rolled a bottle off the table. The inclinometer, the apparatus for testing the roll on board the *Castalia*, never showed more than five degrees of rolling, when other vessels rolled to a very great extent. Now, with regard to what Mr. Liggins said as to not being able to see anything in the channel between the two hulls when the vessel came into Dover, and that it was entirely choked up with water, the fact is, that the paddles block up the passage right across. The distance across I think is 22 ft., and the paddles are 21 ft. of the whole distance across, so that it is impossible that could be seen through unless you could see through the whole thickness of the paddle-box.

Mr. Liggins: I did not say I could not see through, but what I said was that the channel was blocked between the two. The trunk between the two vessels was choked with water from the cut-water to the paddle-wheel. Captain Dicey and I cannot be talking of the same day, because there is a gentleman here who was a passenger on the day to which I have alluded, and he can confirm my statement.

Captain Dicey: The vessel only rolled 5 degrees when other vessels rolled much more—14 or 15—that is a fact that cannot be denied. In the report of the London, Chatham, and Dover Company which they sent out to all the shareholders printed in a small pamphlet, they say that the *Calais-Douvres* always made a faster passage than the *Maid of Kent*, their best vessel. The *Maid of Kent* was always looked on as one of the fastest vessels in the Channel, but it so happens that the *Maid of Kent* on one occasion crossed over with the *Calais-Douvres*, and the *Calais-Douvres* made the passage in something like ten minutes less than the *Maid of Kent*. Those facts speak for themselves. With regard to one or two occasions when she made rather a long passage, that was entirely owing to the steering apparatus.

They steered by steam, and the steam gear frequently got out of order, and detained the vessel both on the trial trip and on two or three occasions afterwards. Besides, I can safely say she never went into the Downs more than twice I think it was, and Captain Pittock says she was a vessel he would not hesitate for a moment about being out in, in a gale.

A Visitor: Mr. Chairman, allow me to say a few words in confirmation of what my old friend Captain Dicey has said. I myself was a passenger in the *Calais-Douvres* no less than seven times during the month of September last. I have had to do with Captain Dicey in different parts of the world—in India—and ever since I have known him very well. I joined him as much from friendship as speculation with regard to the *Castalia*, in which unfortunately I am a shareholder. I must say that a more comfortable vessel in a gale of wind no man who has gone round the Horn or the Cape of Good Hope need be in, and had she capacity for carrying coals, I certainly would be glad to form one of the crew to go a pleasure trip round the world in the *Castalia* or the *Calais-Douvres*. As to the unfitness of these vessels to cross in bad weather, I must distinctly contradict what I have heard said by a gentleman whose name I do not know, that there could be anything like disagreeableness in her. On one occasion when she crossed there were more than 500 people on board, and many of them were ill. In that case there were some relatives of my own, who before they left Calais were ill while the ship was lying in quiet water, and they were certainly very ill on their way across, as also were many others. I have knocked about the world in all portions of it, and in all sorts of seas, but I certainly felt no necessity to be ill. A great deal of spray came over the vessel, which any body could see, but it does not require a sailor to tell you that if there is a large vessel which cannot possibly roll, the sea hits the side of that as it would do the Dover Admiralty Pier, and the spray must fly in, if the ship cannot give. Water takes its shortest route, as Mr. Scott Russell explained last night—water will rise to the surface by the shortest route, and will come down by the shortest route. The comparison of the two ships of course one has nothing to do with. I can quite verify what Mr. Liggins said about the uncomfortable feeling in the *Calais-Douvres* on going down below on such a day with everything open: the heat was perfectly unbearable. That I never experienced on board the *Castalia*. It may be there is something in the form of the *Calais-Dovres* which makes the heat in her so much greater in excess of what it is in the *Castalia*, but you cannot avoid it, and by the enormous sausage-boxes there are on the top of her boilers, which are very nearly as large as the boilers, a huge mass of steam is kept in the *Calais-Douvres* that does not exist in the *Castalia*. As to the comfort on deck, I think it does not require one who has crossed so often in her as myself, or the word of Captain Dicey, or the word of Captain Pittock, whom I know well, who I am quite certain will speak of both the vessels with the highest praise, to show that if 400 people can go on board that vessel with plenty of air, they must have travelled in greater comfort than on the deck of a vessel that does not represent one-sixth of the superficial space. The deck must convince you all of it. As to the great comfort of the twin ship, that is thoroughly secured. As to the speed, that is quite another matter. Mr. Harland has explained from a shipbuilder's point of view, I should say, that the whole arrangements were very unsatisfactory, but I think myself, we need not despair of having a vessel presenting such a very large skin friction being propelled at as good a pace as a vessel with one-half, whether we get it by putting an enormous excess of horse-power, as Mr. Leslie has done in the *Calais-Douvres*, or by some other adaptation, whether paddles or screws, or something we know nothing about at present; but I feel convinced that attempts should be made to drive this form, because though we hope large harbours will be made both on this side and on the French side, still certainly our grandchildren will not have an opportunity of crossing over in such vessels as go between Holyhead and Kingstown. The time it will take to build those harbours will see us all in another state of existence, and I think we must not

despair of being able to propel these twin vessels at a speed far in excess of any boats, whether they run from Calais to Dover, or across the great Atlantic. As to the form of this vessel, I may remark that it is very old. I may mention that I am acquainted with a gentleman who was in this room this morning, a well-known engineer in the north of England, a relative of mine, who made experiments forty years ago on the hulls of twin ships. Some of the drawings of those ships I have seen. They commenced with the single hull cut in half with straight sides, and he then gradually altered the form, putting in slightly developed bows on the inner side, and he went on until he got both bows and both hulls equal. He found by actual experiment that the straight line was that which with the same power gave the greatest speed. I think from what we have heard of the easy way in which the *Castalia* is driven with about 1,220 horse power nominal as compared with the enormous power of the *Calais-Douvres*, if the experiments were carried further by separating these large models for every inch that they were separated from being one hull, it would be found that the speed decreased with the same power.

Mr. G. C. Mackrow: I will say, gentlemen, that I had no thought of bringing forward this paper to advocate the system. It was merely to present to the Institution the results that I have noticed in connection with her. I think the system itself is before the public, and everyone must judge for himself of its value. With regard to Mr. John's remarks, I may say I was very glad, of course, when so many conflicting opinions were abroad as to the construction of this vessel, to have Messrs. Lloyd's at my back. Everyone who came to see the vessel at the works during her construction expressed their fears that we should come to grief sooner or later, and of course it was a responsible position for the servant of a large company to be placed in. And I was very glad to find that I had the full support of Messrs. Lloyd's in what we had undertaken. Then, with regard to Mr. Liggins' remarks, I believe Captain Dicey and others have fully met them. I might say, with regard to the remarks Mr. Harland has made as to the cutting away of the floats, that was the very first thing we did after the first time we went down the river. The very night we brought up at Gravesend, the first thing we did was to cut away some 6 or 8 inches from each end of the floats, as it was feared that the water was jamming between the ends of the floats and the ship's side. This cutting process went on until eventually we found her fitted with those patent floats of Mr. Aston, which consist of two 6-inch angle irons placed vertically side by side. I was not present at the trial, but it was reported that she had made the same speed as with the ordinary horizontal floats. With regard to Mr. Parker's remarks, or rather inquiry, as to who was responsible, I might say that Messrs. Blyth had undertaken originally the whole contract for the ship, and for the engines. Some experiments had been tried, I think by the Channel Steamship Company, on two vessels lashed together above bridge, but I myself have never attached much importance to those experiments, for this reason, that they were not tried under the same conditions as in the *Castalia*, inasmuch as you have wheels on the outside as well as the inside, and I think, as I have endeavoured to show in the paper, that it is owing to the wheels being placed on the inside that we meet with this enormous amount of resistance. I should be very glad indeed to see the engines reversed, and see those wheels simply placed on the outside of the hull. No one, I fear, will undertake any such experiment, but it certainly would be very satisfactory. I think we should find then no very great difference between the performance of this vessel and an ordinary vessel. Mr. Harland has shown us clearly at what depth below the hull of the vessel the particles of water are found in motion in connection with the hull. If you notice, in the paper I have drawn attention to what I believe is the action taking place in an athwartship direction, and if that is the case, and we carry those particles of water down to 3 feet below the bottom of the hull, it actually amounts to an increase of the midship section of the vessel to another 3 feet of depth. Of course Mr. Leslie is deserving of all the credit that Mr. Parker gives

to him, but certainly you must bear this in mind, that Mr. Leslie had the *Castalia* to experiment upon, which we had not. I think I have met the inquiry as to who was responsible. Of course we were hoping against hope. It was an experimental vessel. We undertook to build it entirely from a commercial point of view, and certainly when it was proposed to us we felt very timid, and we have sometimes to refuse to entertain propositions when they are so exceedingly wild; but seeing how desirable it was to have steamers suitable for the passage across the Channel, we felt disposed to run some risk; but it was not in the power of the Directors of the Thames Iron Company to expend large sums of money upon costly experiments involving considerable outlay, especially as it was in a matter that they themselves were to derive no especial benefit from. Such experiments devolved upon the Channel Steamship Company. I can only thank you, gentlemen, for the kind way in which you have received the paper, and say this, that while I gave notice under pressure that I would contribute a paper some few months ago, I really left it until the last moment, but I should have been very glad indeed if a paper of more value had been forthcoming. When I heard towards the close of the time that nothing presented itself of more value, as the Council judged, I then had barely time to prepare the plans and the paper; I have, however, presented it to you to-day in the best possible form that I could.

The President: We must all feel, I am sure, very much obliged to Mr. Mackrow for the very interesting paper which he has contributed, and for the interesting discussion which it has elicited from the gentlemen present.

ON SOME OF THE CAUSES OF FAILURE OF CRANK-SHAFTS IN MARINE ENGINES.

By J. T. Milton, Esq., Member.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B , D.C.L., President, in the Chair.]

Of all parts of marine engines that which requires to be renewed most frequently is the crank-shaft. The majority of break-downs of machinery at sea are caused by broken shafts, whilst those that break at sea are a small portion of the total number which are condemned and replaced owing to serious flaws being discovered in them. The great expense and inconvenience arising from the detention of vessels when being fitted with new shafts have led to the adoption by most engineers, in large engines, of crank-shafts made in two halves, each half being a duplicate of the other, so that in the event of a break-down one half only has to be renewed; and it is also the practice of some engineers, not only to make their shafts in similar halves, but to make each half reversible, so that in the event of a flaw appearing in any bearing, it can be turned into the forward bearing, upon which no twisting strain comes. Now, there must be some cause or causes for this great mortality amongst shafts, and any discussion on this subject which would reveal the cause or causes, and at the same time suggest a remedy for the evil, would be of great interest.

Crank-shafts might fail through being originally made of insufficient diameter for the work for which they are designed, and in at least one noted case this was no doubt the reason; but in the majority of marine engines this is not the case, yet they sometimes break after only a few months' work. Another cause which might lead to their failure is bad forging, and although the fractures of shafts have in some instances shown either that the material was bad or that the piling of the iron was defective, yet in very many broken shafts the material is thoroughly good. If the material is good, and the shaft is of proper dimensions, and yet comes to grief, it must be through its being subjected to some specially severe strains, greater than those due to ordinary working. It is to the consideration of some of these strains that this paper is devoted.

That some abnormal strain is the cause of flaws occurring in shafts is borne out by the fact that in many shafts flaws are observed, marked at the ends, and carefully watched to see if they extend, and the shafts then run for months, and sometimes even for years, at full power without any extension of the flaws, whilst the shaft must, in its flawed state, be weaker than before.

Most of the flaws for which crank-shafts are condemned occur at the angle between the

web of the crank and the journal or crank pin, at the place where the forging is most likely to be defective, and they are evidently produced more by bending strains than by twisting; at these places the change of form of the shaft throws great local stresses on the material, and there can be no doubt that if these parts are made with a large radius the strength of the shaft is materially increased.

Amongst the causes which tend to throw great strains on the shafting may be mentioned the presence of water in the cylinders, slackness of the brasses, and also the bearings being out of line. It is very difficult to calculate the amount of strain produced by the first of these causes quantitatively, as it depends upon the pressure which would open the relief valve or force the slide valve off the face, and thus afford an outlet for the water, and it is only too common to find the relief valves either set fast in their seats, or their springs rusted up solid, but on the supposition of the crank being very near the centre, the piston and cylinder cover, or bottom, being absolutely rigid and there being no outlet for the water, the leverage of the other engine would be sufficient to produce an almost infinite force in the connecting rod, so that it is easily seen that, practically, water in the cylinders might produce an excessive bending strain on the shaft.

It is also difficult to estimate quantitatively the strain produced by slackness of the brasses, but in most engines the amount of steam cushioning is sufficient to reverse the direction of the pressure on the piston before the end of the stroke, and the change of pressure in this case being gradual the slackness of the brasses is taken up gradually. In some cases, however, where there is no cushioning the reversal of pressure takes place at the end of the stroke by the full steam pressure coming on the piston suddenly, and a blow, and consequently an impulsive strain, is thrown on the shaft, the intensity of the strain depending upon the amount of slackness of the brasses.

The effect of the bearings being out of line is to throw the greater part of the supporting forces of the shaft upon one particular bearing; and although it is improbable that a very unequal distribution of the forces at the bearings would long escape attention, owing to the bearings becoming hot, yet in the case of a hot bearing, to which all engines are at times liable, the usual practice of slacking back the nuts of the cap entirely relieves the bearing of all pressure on the up stroke, and for the up stroke at least the shaft is in the position of having one bearing taken entirely away, whilst if two bearings were to heat at the same time the case would of course be rendered still more serious.

In endeavouring to calculate the strains actually coming upon a crank-shaft, we meet with a little difficulty. The forces acting upon the crank-pins are found directly from the indicator diagrams, and from them the twisting moments on the journals are very easily calculated, but the twisting on the crank-pins, and also the bending strains on both the pins and journals, depend also upon the forces exerted at the bearings; even when the bearings are absolutely true these supporting forces are difficult if not absolutely impossible to calculate exactly, owing to the want of continuity of the shaft itself, but when the bearings are slightly out of line the problem is still further complicated.

I have made some calculations of these strains in the cases of five engines of different types, for the purpose of ascertaining the extent to which they might be increased by the bear-

ings being out of line. In order to do this, I have found the strains on the various journals and crank-pins under ordinary conditions, and also under the conditions that would exist if one of the bearings were taken away, this representing a limit beyond which the effect of untrue bearings is unlikely to proceed, whilst, as I have noted, it is by no means an unlikely condition for the shaft to be in. The strains are represented by the diagrams, the base line representing the angular position of the crank-shaft, in all cases starting from the forward crank upon the top centre, whilst the heights of the curves at any ordinate represent on the scale attached, the strain the shaft is subjected to in that position. The curves show, first, the twisting moment; secondly, the bending moment; and thirdly, the twisting moment which would, if acting alone, produce the same strain on the shaft as the bending and twisting moments combined.

In all these cases I have taken into consideration the weight and inertia of the moving parts, and also the obliquity of the connecting-rod, as I find the results very different from those obtained by neglecting them; but in considering the bending moments I have assumed that the forces upon the bearings are concentrated at their centres, instead of being distributed along their length. This gives a result rather greater than would be obtained if we considered them uniformly distributed, but as the same assumption has been made in the case of the bearings being taken away, the ratio of the strains in the two cases is lessened, because distributing the forces has a greater effect when the bearings are close than when they are far apart

I have considered it unnecessary to examine the cases in which the third or fourth bearings are adrift, as they will be practically the same as with the second or first bearing adrift. In all the cases, I have taken the forces at the bearings to be the same as they would be provided the shaft were of uniform section throughout, this being considered to be as near an approximation as could be made. (For a mathematical solution of this problem, see Appendix.)

The first and simplest case that I have taken is that of a single engine, which I will call Engine A. I have taken one of the compound type—in fact, the half of Engine B (high pressure cylinder 48 inch, and low pressure 83 inch diameter; length of stroke 5 ft., steam pressure 70 lbs. per square inch). In this case the forces at the bearings must be nearly alike, and the bending moment on the crank-pin will be half the load on piston by half the span of bearings: the strains in this case are shown in Fig. 1[A], Plate XV. If, however, the effect of the other bearings was to support the shaft, so as to put it in a similar condition, as far as bending is concerned, as a girder fixed at one end and supported at the other, the maximum bending moment is three-fourths of that given above, but it occurs in the aft bearing: the strains on this supposition are shown by Fig. 2[A], Plate XV. The actual strain must lie between these two cases, and must be considerably nearer the first.

If, now, we suppose the first bearing to be taken away, the strains on the after journal are shown by Fig. 3[A], Plate XV.

From these diagrams, it appears that in the best working condition the shaft is strained 1·38 times as much as it would be by the twisting moment acting alone, such a strain as, in fact, comes upon the tunnel shafting, whilst the effect of taking away the support of the forward bearing is to double the strain on the shaft.

The twisting moment on the after bearing, and also on the tunnel shaft, is shown by the curve in Figs. 2[A] and 3[A] (Plate XV.), and from them it appears that in this engine the maximum twisting strain is 2·1 times the mean.

The next case, Engine B, is that of a compound engine with four cylinders, the cranks being at 90°. The high-pressure cylinders are 48 in. diameter, the low 83 in.; length of stroke, 5 ft.; steam pressure, 70 lbs. per square inch. This type of engine might also be taken to represent the simple engine with two equal cylinders, cranks at right angles. Fig. B 1 (Plate XVI.), represents the twisting moments exerted by each engine; the upper curve represents the sum of the moments on the two engines, and is, therefore, the strain on the tunnel shaft. Figs. B 2, B 3, B 4, and B 5 (Plate XVI.), represent the twisting, bending, and also the equivalent twisting moment upon the forward crank-pin, second and third bearings, and after crank-pin when the engine is in good working condition. It will be seen that the greatest strain comes upon the after crank-pin. Figs. B 6, B 7, and B 8 (Plate XVI.), represent the strains upon the second bearing, third bearing, and after pin, on the supposition of the first bearing being taken away: in this case the second bearing is most strained. Figs. B 9, B 10, and B 11 (Plate XVI.), represent the strains upon the forward pin, third bearing, and after pin, upon the supposition of the second bearing being taken away: in this case the forward pin is most strained.

In this engine it appears that the ratio of greatest to mean twisting moments in the tunnel shaft is 1·37 to 1; the crank shaft is subjected to a strain equal to that which would be caused by a twisting moment 1·15 times the greatest twisting moment in the tunnel shaft, when the engines are in good working order; whilst the first bearing, being adrift, causes a strain on the shaft twice as great as that due to ordinary working, and the second bearing adrift causes the shaft to be strained 1·36 times as much as when the bearings are in proper order.

The next case, Engine C (see Plate XVII.), is that of the commonest type of marine engine, viz. compound, with two cylinders and cranks at right angles; and Engine D (see Plate XVIII.) is also of the compound type, but the cranks are at an angle of 130°: in each case there are four bearings to the crank-shaft. Engine C has cylinders of 44 and 72 in. diameter, a stroke of 3 ft. 6 in., and steam pressure of 60 lbs., whilst Engine D has cylinders of 26 and 52 in. diameter, a stroke of 2 ft. 6 in., and steam pressure of 80 lbs. per square inch.

The figures with the same numbers represent the strains on the same parts of the shaft as in Engine B, under the same conditions. It will be noticed that in Engine C the ratio of maximum to mean twisting moment is 1·48 to 1, whilst in Engine D the ratio is 1·77 to 1. In both engines, in ordinary working, the after crank-pin is the part which is most strained: in Engine C it is strained 1·11 times that due to greatest twisting moment, while in Engine D it is strained 1·05 times as much as the tunnel shafting. If the first bearing is taken away the shafts are strained in Engine C to twice, and in Engine D to 1·46 times that due to ordinary working, while slacking second bearing increases the strain on the shaft in the ratios of 1·43 and 1·06 times in the two engines.

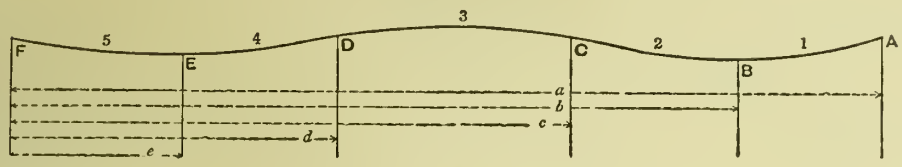
The next case, Engine E (see Plate XIX.), is also that of a compound engine with two cylinders; the cranks in this case are set at an angle of 135°, but there are only three bearings to the crank-shaft; the diameters of the cylinders are 34 and 60 in., the stroke is 3 ft., and steam pressure 60 lbs. per square inch. Fig. E 1 represents the twisting moments or tunnel shafting, the ratio of maximum to mean being in this case 1·94 to 1. Figs. E 2, E 3, and E 4 represent the strains on the forward pin, middle bearing, and after pin, in ordinary working; in this case also the after pin is the greatest strained, it having to sustain 1·21 times the moment on the tunnel shafting. Figs. E 5 and E 6 represent the strain on the middle bearing and after pin when the first bearing is taken away. In this case, we see that the shaft is strained twice as much as in ordinary working, whilst when the middle bearing is adrift, the strains being represented by Figs. E 7 and E 8, the shaft is strained 1·28 times as much as in ordinary working.

These results show what a great influence upon the strains acting on a crank-shaft the position and truth of the bearings exert; and in considering these strains it is to be borne in mind that it often happens that other causes are also at work increasing the strains on the shafting at the same time: for instance, we might have slack brasses in the connecting rods, hammering on the shaft at the same time that the bearings are adrift. It must also be noted that they are not strains which come only a few times on the shaft, for with the engines making 60 revolutions per minute they would be repeated 1,000 times in seventeen minutes, whilst in the case of the bearings being set out of line, the strains this would cause (reckoning it only once per revolution) would be repeated upwards of 86,000 times in the course of the day, when running at 60 revolutions per minute.

	Ratio of Maximum to Mean Twisting Moments.	Ratio of greatest stress on Crank Shaft to that on Tunnel Shaft in proper working order.	Ratio of greatest stress on Shaft with first Bearing taken off to that when in proper working order.	Ratio of greatest stress on Shaft with second Bearing taken off to that when in proper working order.
Engine A.....	2·1	1·38	2·0	—
Engine B.....	1·37	1·15	2·0	1·36
Engine C.....	1·48	1·11	2·04	1·43
Engine D.....	1·77	1·05	1·46	1·06
Engine E.....	1·94	1·21	2·05	1·28

APPENDIX.

In a girder of uniform section, supported on four piers and loaded in the two end spans, to determine the supporting forces.



LET A B C D E F be the girder, A, C, D, F, the piers, B and E the points of application of the loads.

Call the supporting forces A, C, D and F, and let B and E represent the loads, all being measured positive in same direction.

Let $AF = a$, $BF = b$, $CF = c$, $DF = d$, $EF = e$; take F as origin, and F A as axis of x.

We shall have the equations to Moments, Inclination and Deflection in span E F involving all the forces, A, B, C, D and E, and the spaces a, b, c, d, and e; those in span D E will be the same equations with the terms involving E and e omitted, and so on.

The Integration constants will be different in the different spans, and are represented by the suffixes to L and K, L_4 and K_4 referring to span E D.

In the following equations all of these constants have been determined, but to determine the supporting forces it is not necessary in individual cases to work them all out.

We have equation to bending moment at point x.

$$M = A(a - x) + B(b - x) + C(c - x) + D(d - x) + E(e - x) = \frac{d^2 y}{d x^2} \text{E.I.}$$

$$\therefore \frac{d y}{d x} . \text{E.I.} = A\left(a x - \frac{x^2}{2}\right) + \ldots + E\left(\frac{e x - x^2}{2}\right) + K \quad . \quad . \quad . \quad (\alpha),$$

K being the integration constant.

$$y . \text{E.I.} = A\left(\frac{a x^2}{2} - \frac{x^3}{6}\right) + \ldots + E\left(\frac{e x^2}{2} - \frac{x^3}{6}\right) + K x + L \quad . \quad . \quad . \quad (\beta),$$

We may omit E.I. throughout, as we are not required to find numerical values.

These are the general equations; those for the particular spans are obtained from them by the omission of all terms referring to forces or distances between these spans and the origin, as explained above.

Now in span 5, if

$$x = 0, \ y = 0 \ \therefore \text{ from equation } (\beta) \ L_5 = 0 \ . \quad . \quad . \quad . \quad (1)$$

also, if

$$x = e, \ y = y_e;$$

$$\therefore y_e = A\left(\frac{a e^2}{2} - \frac{e^3}{6}\right) + \ldots + E\left(\frac{e^3}{2} - \frac{e^3}{6}\right) + K_5 e + L \ .$$

In span 4, if $x = e,\ y = y_e.$

$$y_e = A\left(\frac{a\,e^2}{2} - \frac{e^3}{6}\right) + \ldots + D\left(\frac{d\,e^2}{2} - \frac{e^3}{6}\right) + K_4 e + L_4;$$

$$\therefore\ E\frac{e^3}{3} + K_5 + L_5 e = K_4 e + L_4 \quad . \quad . \quad . \quad . \quad (2).$$

Also in span 4, if $x = d,\ y = y_d,$

$$A\left(\frac{a\,d^2}{2} - \frac{d^3}{6}\right) + \ldots + D\left(\frac{d^3}{2} - \frac{d^3}{6}\right) + K_4 d + L_4 = y_d.$$

In span 3, when $x = d,\ y = y_d$

$$A\left(\frac{a\,d^2}{2} - \frac{d^3}{6}\right) + \ldots + C\left(\frac{c\,d^2}{2} - \frac{d^3}{6}\right) + K_3 d + L_3 = y \quad . \quad . \quad . \quad (\gamma).$$

$$\therefore\ D\frac{d^3}{3} + K_4 d + L_4 = K_3 d + L_3 \quad . \quad . \quad . \quad . \quad (3).$$

In span 3, when $x = c,\ y = y_c,$

$$A\left(\frac{a\,c^2}{2} - \frac{c^3}{6}\right) + \ldots + \frac{C\,c^3}{3} + K_3 c + L_3 = y_c;$$

and in span 2, when $x = c,\ y = y_c,$

$$A\left(\frac{a\,c^2}{2} - \frac{c^3}{6}\right) + B\left(\frac{b\,c^2}{2} - \frac{c^3}{6}\right) + K_2 c + L_2 = y_c \quad . \quad . \quad . \quad . \quad (\delta),$$

whence

$$C\frac{c^3}{3} + K_3 c + L_3 = K_2 c + L_2 \quad . \quad . \quad . \quad . \quad (4).$$

Similarly we obtain

$$B\frac{b^3}{3} + K\ b + L_3 = K_1 b + L_1 \quad . \quad . \quad . \quad . \quad (5),$$

and in span 1, since if $x = a,\ y = 0,$

We have

$$A\frac{a^3}{3} + K\ a + L_1 = 0 \quad . \quad . \quad . \quad . \quad . \quad (6).$$

From equation (a) we obtain—

In span 5, if

$$x = e,\ \frac{d\,y}{d\,x} = i\ ,$$

$$i_e = A\left(a\,e - \frac{e^2}{2}\right) + \ldots + E\left(\frac{e^2}{2}\right) + K_5.$$

In span 4, if

$$x = e,\ \frac{d\,y}{d\,x} = i_e,$$

$$i_e = A\left(a\,e - \frac{e^2}{2}\right) + \ldots + D\left(d\,e - \frac{e^2}{2}\right) + K_4,$$

whence

$$\frac{E\,e^2}{2} + K_5 = K_4 \quad . \quad . \quad . \quad . \quad (7),$$

Similarly by equating results for other spans, we get

$$\frac{D\,d^2}{2} + K_4 = K_3 \quad . \quad . \quad . \quad . \quad (8),$$

$$\frac{C\,c^2}{2} + K_3 = K_2 \quad . \quad . \quad . \quad . \quad (9),$$

$$\frac{B\,b^2}{2} + K_2 = K \quad . \quad . \quad . \quad . \quad (10).$$

These ten equations enable us to express $K_1 \quad . \; K_5$, $L_1 \; . \; . \; . \; L_5$ in terms of A . . E, $a \; . \; . \; . \; e$.

From (2) and (7) we obtain

$$L - L_4 = \frac{E\,e^3}{6},$$

from (3) and (8),

$$L_4 - L_3 = \frac{D\,d}{6},$$

similarly,

$$L_3 - L_2 = \frac{C\,c^3}{6},$$

$$L_2 - L_1 = \frac{B\,b^3}{6},$$

also from (1),

$$L_5 = 0,$$

whence also,

$$L_4 = -\frac{1}{6}E\,e^3,$$

$$L_3 = -\frac{1}{6}(E\,e^3 + D\,d^3),$$

$$L_2 = -\frac{1}{6}(E\,e^3 + D\,d^3 - C\,c^3,$$

$$L_1 = -\frac{1}{6}(E\,e^3 + D\,d^3 + C\,c^3 + B\,b^3).$$

From (6) we have

$$K_1\,a = -L_1 - \frac{A\,a^3}{3};$$

$$\therefore K_1 = \frac{1}{6\,a}(E\,e^3 + \; . \; . \; . \; + B\,b^3 - 2\,A\,a^3).$$

and from (10), (9), (8), and (7), K_2, K_3, K_4, and K_5 can be determined.

These equations hold good whether points D and C are in line with A and F or not; or in other words, the integration constants are the same whether the girder is just touching the piers D and C before being loaded or not.

To obtain the actual values of supporting forces in the case where the unloaded girder would just touch the piers we have in equations (γ) and (δ)

$$y_d = 0 \text{ and } y_e = 0,$$

whence

$$A\left(\frac{a\,d^2}{2} - \frac{d^3}{6}\right) + \ldots + C\left(\frac{c\,d^2}{2} - \frac{d^3}{6}\right) + K_3 d + L_3 = 0 \quad . \quad . \quad (\gamma),$$

and

$$A\left(\frac{a\,c^2}{2} - \frac{c^3}{6}\right) + B\left(\frac{b\,c^2}{2} - \frac{c^3}{6}\right) + K_2 c + L_2 = 0 \quad . \quad . \quad . \quad . \quad (\delta),$$

also we have the conditions of equilibrium

$$A + B + C + D + E + F = 0 \quad . \quad . \quad . \quad . \quad (\epsilon),$$

$$A\,a + B\,b + C\,c + D\,d + E\,e = 0 \quad . \quad . \quad . \quad . \quad (\zeta),$$

making four equations to determine the four quantities.

Example.—Suppose $d = 2\,e$, $c = 4\,e$, $b = 5\,e$, $a = 6\,e$, we have

$$L_3 = -\frac{e^3}{6}(E + 8\,D),$$

$$L_2 = -\frac{e^2}{6}(E + 8D + 64\,C\,s)$$

$$K_1 = \frac{e^2}{36}E + 8\,D + 64\,C + 325\,B - 432\,A);$$

$$\therefore \quad K_2 = \frac{e^2}{36}(E + 8\,D + 64\,C - 325\,B - 432\,A),$$

$$K_3 = \frac{e^2}{36}(E + 8\,D - 224\,C - 325\,B - 432\,A).$$

Substituting these values, (γ) and (δ) become

$$\left.\begin{array}{rl} & 240\,A + 169\,B + 104\,C + 16\,D + 2\,E = 0 \\ \text{and} & 192\,A + 122\,B + 64\,C + 8\,D + E = 0 \\ \text{also} & 6\,A + 5\,B + 4\,C + 2\,D + E = 0 \end{array}\right\}$$

whence,

$$A = -\frac{48\,B + 3\,E}{120}, \qquad C = -\frac{87\,B - 18\,E}{120},$$

$$D = -\frac{87\,E - 18\,B}{120}, \quad \therefore \; F = -\frac{48\,E + 3\,B}{120}.$$

As a second case we will now assume the support at A withdrawn.

The equations will be the same with the terms A and a omitted, and there will be no terms involving suffix 1.

We shall have as before,

$$\left.\begin{array}{l} \dfrac{E\,e^3}{3} + K_5 e + L_5 = K_4 e + L_4 \\ \dfrac{D\,d^3}{3} + K_4 d + L_4 = K_3 d + L_3 \\ \dfrac{C\,c^3}{3} + K_3 c + L_3 = K_2 c + L_2 \end{array}\right\} \quad . \quad . \quad . \quad . \quad (11),$$

and

$$\left.\begin{aligned} \frac{E\,e^3}{2} + K_5 &= K_4 \\ \frac{D\,d^2}{2} + K_4 &= K_3 \\ \frac{C\,c^2}{2} + K &= K_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (12),$$

and from these we get as before,

$$L_5 - L_4 = \frac{E\,e^3}{6},$$

$$L \;\; - L \;\; = \frac{D\,d^3}{6},$$

$$L_3 - L \;\; = \frac{C\,c^3}{6};$$

and also as before,

$$L_5 = 0,$$

whence

$$L_4 = -\frac{E\,e^3}{6},$$

$$L_3 = -\frac{1}{6}(E\,e^3 + D\,d^3),$$

$$L_2 = -\frac{1}{6}(E\,e^3 + D\,d^3 + C\,c^3),$$

Also from equation (δ) we get
since $y = 0$ when $x = c$, and in this case $A = 0$,

$$B\left(\frac{b\,c^2}{2} - \frac{c^3}{6}\right) + K_2\,c + L_2 = 0\,;$$

$$\therefore\; K_2\,c = \frac{1}{6}(E\,e^3 + D\,d^3 + C\,c^3) - B\left(\frac{b\,c^2}{2} - \frac{c^3}{6}\right);$$

$$\therefore\; K_2 \;\; = \frac{1}{6\,c}(E\,e^3 + D\,d^3 + Cc^3) - B\left(\frac{b\,c}{2} - \frac{c^3}{6}\right),$$

and from equations (12) we may obtain K_3, K_4, and K_5.

To obtain actual values of supporting forces we have as before,

equation (γ)

$$\left.\begin{aligned} B\left(\frac{b\,d^2}{2} - \frac{d^3}{6}\right) + C\left(\frac{c\,d^2}{2} - \frac{d^3}{6}\right) + K_3\,d + L_3 &= 0 \\ B\,b + C\,c + D\,d + E\,e &= 0 \\ \text{and } B + C + D + E + F &= 0 \end{aligned}\right\} \quad . \quad . \quad (13)$$

to find C, D, and F.

Example.—Same girder as before,

$$L_3 = -\frac{e^3}{6}(E + 8\,D),$$

$$K_2 = \frac{1}{6\,c}(E\,e^3 + Dd^3 + Cc^3) - B\left(\frac{b\,c}{2} - \frac{c^2}{6}\right),$$

$$= \frac{e^2}{24}(E + 8\,D + 64\,C) - \frac{B\,e^2}{6}(60 - 16),$$

$$= \frac{e^2}{24}(E + 8\,D + 64\,C - 176\,B),$$

$$K_3 = K_2 - \frac{C\,c^2}{2} = \frac{e^2}{24}(E + 8\,D - 128\,C - 176\,B),$$

therefore equations (13) become

$$\frac{26}{3}B + \frac{20}{3}C + \frac{1}{12}(E + 8D - 128C - 176B) - \frac{1}{6}(E + 8D) = 0,$$

which reduces to

$$\left.\begin{array}{rl} & 72B + 48C + 8D + E \quad = 0 \\ \text{and} & 5B + 4C + 2D + E \quad = 0 \\ \text{and} & B + C + D + E + F = 0 \end{array}\right\},$$

from which we obtain

$$C = \frac{3E - 52B}{32}, \quad D = \frac{24B - 22E}{32}, \quad F = -\frac{4B + 13E}{32}.$$

The next case to consider is where the second pier or support C is wanting. This is also the case of a girder supported on three piers, and loaded in each span.

We may conveniently deduce this case direct from the general case, by assuming the support C to move indefinitely near to D, that is let $c = d$, and the supporting force will then evidently be $C + D$.

We then get the equations

$$L_5 = 0,$$

$$L_4 = -\frac{1}{6}(E e^3),$$

$$L_3 = -\frac{1}{6}(E e^3 + D d^3),$$

$$L_2 = -\frac{1}{6}(E e^3 + D d^3 + C d^3),$$

$$L_1 = -\frac{1}{6}(E e^3 + D d^3 + C d^3 + B b^3),$$

$$K_1 = \frac{1}{6a}(E e^3 + D d^3 + C d^3 + B b^3 - 2A a^3),$$

$$K_2 = K_1 - \frac{B b^2}{2},$$

And to obtain actual values of A, $C + D$ and F we have (Note: (γ) and (δ) become identical, having reference to same point)—

$$A\left(\frac{a d^2}{2} - \frac{d^3}{6}\right) + B\left(\frac{b d^2}{2} - \frac{d^3}{6}\right) + K_2 d + L_2 = 0 \quad . \quad . \quad . \quad (\delta),$$

$$\text{and } A a + B b + C d + D d + E e = 0,$$

$$A + B + C + D + E + F \quad = 0,$$

three equations from which to obtain A, $(D + C)$ and F.

Example as before, $a = 6e$, $b = 5e$, $c = d = 2e$,

$$L_2 = -\frac{e^3}{6}\{E + 8(D + C)\},$$

$$K_1 = \frac{e^2}{36}\{E + 8(D + C) + 125B - 432A\},$$

$$K = \frac{e^2}{36}\{E + 8(D + C) - 325B - 432A\},$$

(δ) becomes

$$\frac{32}{3}A + \frac{26\,B}{3} + \frac{1}{18}\{E + 8(D + C) - 325\,B - 432\,A\} - \frac{1}{6}\{E + 8(D + C)\} = 0,$$

or

$$\left.\begin{aligned} 240\,A + 169\,B + 16\,(D + C) + 2\,E &= 0 \\ 6\,A + \quad 5\,B + \quad 2\,(D + C) + \quad E &= 0 \\ A + B + (D + C) + E + F \quad &= 0 \end{aligned}\right\},$$

whence

$$A = \frac{2\,E - 43\,B}{64}, \quad (D + C) = -\frac{38\,E + 31\,B}{64}, \quad F = \frac{10\,B - 28\,E}{64}.$$

Another Example.—Let $a = 10$ ft., $b = 8$ ft., $c = d = 5$ ft., $e = 2$ ft., which is a special case of one bearing at centre.

Here

$$L_2 = -\frac{1}{6}\{8\,E + 125\,(D + C)\}$$

$$K_1 = \frac{1}{60}\{8\,E + 125\,(D + C) + \quad 512\,B - 2000\,A\},$$

$$K_2 = \frac{1}{60}\{8\,E + 125\,(D + C) - 1408\,B - 2000\,A\},$$

and the equations become

$$\frac{625}{6}A + \frac{475}{6}B + \frac{1}{12}\{8\,E + 125\,(D + C) - 1408\,B - 2000\,A\} - \frac{1}{6}\{8\,E + 125\,(D + C)\} = 0,$$

or

$$\left.\begin{aligned} 750\,A + 458\,B + 125\,(D + C) + 8\,E &= 0 \\ 10\,A + \quad 8\,B + \quad 5\,(D + C) + 2\,E &= 0 \\ A + B + (D + C) + E + F \quad &= 0 \end{aligned}\right\},$$

whence

$$A = \frac{21\,E - 129\,B}{250}, \quad D + C = -\frac{142}{250}(B + E), \quad F = \frac{21\,B - 129\,E}{250}.$$

DISCUSSION.

Mr. A. C. Kirk said: My Lord and gentlemen, at this advanced period of the evening it would ill become me to occupy much time, but I cannot allow this paper to pass without expressing our indebtedness to Mr. Milton for a paper which must have cost him a vast amount of labour on a most important subject—a paper which will be at once suggestive and useful for reference. I cannot pretend in a short perusal to follow Mr. Milton, or to criticise in any way his calculations, but the mortality of crank shafts is a very serious matter indeed. One frequent cause of failure is that the crank shafts are too small originally to meet a reasonable amount of carelessness and the work to which they are

subjected. No doubt Mr. Milton, in discussing the case of bearings out of line, has hit the chief cause of crank shafts giving way. I have a very strong idea that if shipowners would lift crank shafts once a year, and put them in line, we should have fewer broken crank shafts. In regard to the proper size of crank shafts, I speak relatively, if a 10-inch shaft is such that it will work for a very long time, my own experience would confirm this—that a 7½-inch shaft would scarcely work a year, and something between an 8 and 9-inch shaft would work from three to four years. The reason of such a wide margin is due to what Mr. Milton has said, the frequently applied strains, or rather the too frequently applied deflections to which the shafts are subjected in working.

Mr. J. Macfarlane Gray: My Lord and gentlemen, I cannot express how much I value this paper, but of course, as Mr. Kirk has said, we cannot take it all in just at once. With regard to straightness of shaft, I think that far too little attention is paid to that, not only by shipowners, but by those whom Mr. Kirk represents—the makers of engines.

Mr. Kirk: No.

Mr. Macfarlane Gray: I think the designers of engines might make engines now in such a way as that the owner could see for himself whether the shaft is straight or not. The rapid extension of the microphone to the indication of all sorts of varying conditions of stress may lead to its application in some simple way to indicate continuously the straining of a crank shaft.* The present system of taking "leads" off crank shafts, using the pedestal bolts to tighten down the top brass upon the lead wire, seems to me to involve an error. The "leads" for the adjustment of the liners ought to be taken by screwing upon *temporary bolts which pull upon the bottom brass,* not on the bed-plate, and bring the two half brasses together. After the liners are stripped to the proper thickness, as found by using these bolts, other leads should be taken off liners and shaft, using the pedestal bolts this time, and it may now be found that the liners are apparently too thin; that will indicate that the bottom brass requires to be packed up by that amount. This indication of unfairness may be missed altogether in the usual adjustment of shafts by the lead wire test. There is a plan adopted by many engineers now of making the bottom brasses without the side flanges or with loose side flanges below, so that you can pack up the bottom brass. It has been a labour of love this, I am sure, with Mr. Milton, or he never could have got through all this work. But in connection with the diagrams which he must have had to enable him to lay this down on paper, I would draw attention to a new kind of diagram which is coming into use. If you take a pair of engines, and instead of working the indicator of each engine by its own crank, you work it by the other one's crank, you get a diagram that can tell you a great deal. The ordinary diagrams are all very well to give you the horse-power of an engine, but the important part of the working of an engine is the crowded part of the diagram at the end where the piston is scarcely travelling; the diagram is crowded together, and you want to see exactly at what point in the stroke the valves are opening; and if you just give it the chord from the other engine, you get a diagram that will puzzle you at first, but which it is quite delightful to go into, because you see on it exactly the gradations in pressure so clearly showing whether the blow is coming on suddenly or not. The method is simple, and when indicator gear is already fitted it costs nothing to do it.

Mr. West: I think we are very much indebted to Mr. Milton for drawing attention to a point which we are very apt to overlook, that is the bending moment on the shaft. It is a common thing to make calculations for the twisting moment, but to neglect the bending moment; and though Professor Rankine has shown us a formula for it in his handbook, it is too much neglected by engineers. Mr. Milton says something with which I can hardly agree with him. He says, "The effect of the

* By one of the microphone details the straining of a crank shaft could be continuously indicated on a dial placed in any part of the ship.

bearings being out of line is to throw the greater part of the supporting forces of the shaft upon one particular bearing." But it appears to me that when eccentricity between the bearings exists, there is something more than a mere alteration of the distance between supports. With each revolution there is an actual bending of the shaft, and fatal mischief may be done before, and apart from any injury that may result from extending the distance between the points of support by slacking back the cap bolts. Mr. Milton says, too, that water in the cylinders will produce theoretically an infinite strain near the end of the stroke But near the end of the stroke you have the lead of the valve, and consequently an escape for the water through the valve opening into the valve-casing, so that I think we cannot in any exact sense speak of the presence of water in the cylinders as producing an infinite strain. Then there is another point which I should just like to ask Mr. Milton a question about, and that is how he has, in those diagrams, converted the bending moment and twisting moment into common terms so as to have them as shown by the red line.

Mr. J. Fortescue Flannery: I am unwilling to let this discussion close without expressing the interest which I, and all engineers, must take in this paper. It is a very practical dissertation upon some of the evils and failures complained of in crank shafts, and points out some means of ascertaining the best remedies for those evils and failures. Then Mr. Milton concludes with a very excellent theoretical investigation of their extent, and the strains which come upon crank shafts. He has gone, as the previous speaker has said, at least two steps further than any other calculator who has published his results in the usual way. A paper very similar to this was published in the "Annual of the Royal School of Science," and that paper dealt in the same way as Mr. Milton has done, or nearly so, with the twisting moments; but, if I recollect rightly, that paper did not give any particulars of the bending moment. The turning moments alone are, of course, of very great importance, but if they do not measure the whole of the strain that comes on the crank shaft, a paper of that kind cannot be said to be complete. No doubt, Mr. Milton's paper, giving as it does the whole of the strains that may, under ordinary conditions, come on crank shafts, may be said to be a very complete dissertation on the subject. There is, however, one paragraph in the paper which Mr. Milton may have to reconsider. It is an expression of opinion merely. He says that if you have a perfectly made and a perfectly forged shaft, and work it for a short time, and see a flaw which must be the result of some abnormal strain, it goes on working without the flaw increasing, showing that the strain was abnormal, and showing that the abnormal strain having once existed did not occur again, and that the flaw is, as a rule, due to that abnormal strain I cannot agree with that conclusion of Mr. Milton's; and I venture to think that, as a rule, the failure of crank shafts, in the majority of cases, is due to imperfect forging, and that the line of reasoning which Mr. Milton takes is not at all supported. The forging may be so imperfect as to allow a flaw to appear at first, but it does not necessarily follow that with the ordinary strain that first flaw having so far satisfied itself (if I may use such an expression) as to open and show, may not then assume a sort of normal position, and go on with the shaft sufficiently strong to bear any ordinary strain that may be brought upon it. I will mention one fact which Mr. Milton is aware of, but does not state in his paper, and that is, that this difficulty with crank shafts has been experienced very largely among the large lines at Liverpool, and that one of the most important lines have resolved to fit their crank shafts in pieces. Instead of half the length of the crank shaft being forged in solid with the axle, it is forged in several pieces. The arm is forged in one and the pin in one, and the parts of the axle in separate pieces, and it is all bored and put together and shrunk on, and it is expected that the weaker part of the shaft, namely, the one underneath the arm, will by that means be made strong enough to bear the strains upon it, for the reason that Mr. Milton has alluded to in his paper. If I am rightly informed, those crank shafts have not yet been tried, but I believe they will be soon tried,

and, probably, by the next meeting of our Institution some gentleman connected with that line of vessels will be able to give us some information about it which will be of great interest. The result of that experiment we should be very glad to have.

Mr. John Ravenhill: My Lord and gentlemen, crank shafts have been the *bête noir* of marine engineers and forge masters for many years. In the first instance, there can be no doubt that there was very great difficulty in getting a forging anything like sound from the forge masters, but by degrees they altered their process of manufacture, and I think, speaking from a somewhat long experience, I may say they have succeeded, and succeeded in a wonderful way, in obtaining soundness in their forgings after a lapse of years. It is true, in the present day, that circumstances are altered and we are using higher pressures. The initial pressure on the piston conveyed down to the crank-pin and the shaft produces a very considerable excess of strain above what the shafts, at the time I speak of, had to undergo. I believe that many and many a crank shaft has been condemned for nothing more than a surface mark. I had the opportunity, some years ago, of seeing a vast number of recorded facts, in connection with shafts working on the other side of the world, in the office of an engineer of one of our principal steamboat companies; and there can be no doubt that after a flaw had been observed to increase for a certain time, and had extended to a certain point, when that point was reached months and years elapsed, and it was never known to extend. That must have been a surface flaw. With reference to this paper of Mr. Milton's, I think he has drawn the attention of all of us to points which, perhaps, we have never yet paid sufficient attention to, but in having drawn our attention to those points, and having talked of the strains produced in crank shafts when out of line, he has not, in doing that, drawn the attention of a large body of men to a point which I think is one of very serious importance. Crank shafts that were perfectly true have broken, and I believe that when engineers afloat reflect on Mr. Milton's paper many a man will say, "I slacked back a certain bearing at a time when it was very hot, and that was the cause of my shaft breaking; it was not from the shaft being out of line, but from the shaft being, if possible, too well fitted in the first instance." I believe that none of us have considered, in the emergency of the moment, in slackening back the bearings, when they are suddenly heated, the violent strains produced at other points, and by our acts at that time, the ultimate danger that may be done to the shaft which is at work. I am sure the thanks of the Institution are due to Mr. Milton for the elaborate diagrams which he has worked out, and the enormous amount of time and labour those diagrams must have taken him to produce.

Mr. A. J. Maginnis: A short time ago I had to make out statistics with regard to crank shafts in large ocean steamers, and, after going over a number of shafts for ten years, I found that the average life of a crank shaft was three years to three and a half years. The principal reason which engineers on board ships have given for the failures of crank-shafts has been, I think, borne out by what Mr. Flannery has stated. The reason they give is a simple one, namely, if the bearings of the shafts and crank pins are left slack enough to work without heating, that must allow of an imperceptible knock or concussion. If that be the case it is easily accounted for, because we are told that continued concussion changes the fibre or nature of iron, or, in other words, crystallizes it. Going over the matter, I find that, during the three years the crank shaft has been in use, taking an average of the transatlantic voyage—the number of concussions which occurred in three years varies from eighty millions to one hundred millions. These are large or small according as the engines worked smoothly or not. Of late years, through the introduction of patent metal, these concussions have been greatly reduced, as the play and freedom allowed in the largest bearings scarcely equals a sixty-fourth at the end of a voyage; so that I think the difficulty will now be got over by the introduction of the shaft that Mr. Flannery spoke of, as in that case the parts of the shaft that are now seriously affected by

this concussion are done away with, because the fibres of the iron in solid shafts are worked through the fillets, and we all know that it is at the fillets of crank shafts that they break. In the new arrangement, if any heating or such like accident occurs, the danger will be of its working slack in the arms; but I think the danger of fracture will be got over.

Mr. J. T. Milton: Two or three questions have been asked about the diagrams, and Mr. Macfarlane Gray asked me if I included the live load. I did include it. I assumed, however, which, perhaps, is not quite true, that the engines were moving uniformly round. That was the assumption I proceeded upon, and then I took the *inertia* of the live load into account. Mr. West has said that with water in the cylinders you would not get any pressure if you had your lead right. In that case you would have no need of having relief valves, yet we know that relief valves, especially in starting engines, open—and open pretty rapidly—so that the lead is not always sufficient to free the engine of water, and sometimes the slides get up a little, or down a little, and then, practically, there is no lead. Then another gentleman has asked how I obtained the red diagram, and, of course, in taking the result of these diagrams (that is a most important point) I have assumed there, as I stated when I was reading the paper, that that was calculated on the plan given by Professsor Rankine in his "Applied Mechanics," Article 325. That is how those diagrams have been obtained; they have been calculated for every twenty degrees of the revolution.

THE LEGAL LOAD LINE, AND SOME REMARKS RELATING TO THE PRINCIPLES OF SAFE FREEBOARD.

By N. E. Porter, Esq., Associate.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

Members of this Institution will be generally aware that, following on the public agitation concerning the over-loading of vessels, clauses* were inserted in the Merchant Shipping Act, 1875, which compelled the owner to mark the load-line on his vessel's side. It has now become the custom to call this mark "Plimsoll," but it appears to have been suggested by Mr. C. M. Norwood, M.P., who stated his intention of proposing it to Parliament when making some remarks on the load-line, at our meeting March 18th, 1875, which will be found in volume xvi. page 44, of the Transactions. And whatever credit may be due to Mr. Plimsoll for the marvellous energy by which he forced the subject of over-loading on public notice, he, wisely or unwisely, declined to accept this proposal as a means of stopping it, or at least accepted it under protest.

The law allows the shipowner to put the mark as low, or as high, as he pleases; the freeboard being measured amidships from the upper surface of the deck, at the side, to the centre of it. And unless its position is challenged by the Board of Trade as unsafe, it becomes for that particular ship the legal limit of immersion. It may possibly be said that these disc centres are often placed much above the point to which it is intended to load. When the law first came into operation this certainly appeared to be the case, and at present instances may be found of their being put six inches higher than it is intended to load to, possibly to avoid a dispute with a grumbling crew as to its exact position when the vessel is not in smooth water, or to allow of extra immersion should the vessel when abroad be offered a freight between ports situated in a tropical region. So far, however, as I have observed, in the great majority of cases it is now the custom to load down to the disc centres when the weight of the cargo requires it.

Shipowners having been thus called upon to practically illustrate their ideas in regard to minimum freeboard, I have taken the opportunity to measure the sides of a number of vessels, and now place before you the results, in the hope that reliable information of this description may prove of some use to those who take an interest in such matters.

* Clauses No. 5 and 6.

There are 90 sailing, and 83 steam vessels, in all 173. Particulars of deck-plan, under-deck tonnage, and dimensions are given to afford an idea of the vessel, and columns are added showing the freeboards with co-efficients of finess worked out by Mr. Martell's method. This will be found in volume xv. of the Transactions, page 102, and being certainly the most thorough, consistent, and practical plan yet published—particularly for the way in which provision is made for the various forms and types of vessels—appeared especially suited for a standard of comparison. Wanting precise information respecting the sheer of these vessels, I have been unable to reduce the freeboards in accordance with the tables, but they are mostly of the ordinary sheer, and the few instances in which the reduction could be claimed would not diminish the freeboard more than from three to six inches.

It will be noted there are six spar-decked and one awning-decked steamers, numbers 163 to 169. In computing the freeboards for these I have treated them as such because they are so called in Lloyd's Register, but being a class of vessels that vary much in the strength of their upper structure the freeboards given by this method must not be too rigidly relied upon, and I have no doubt Mr. Martell could explain and modify the discrepancies which appear had he an opportunity of seeing the vessels.

This remark would probably apply to some other of the instances given, for it is only fair to bear in mind the author of these tables intended them principally for reference, apparently basing the more exact apportionment of the height of side required on practical considerations of the particular build and type of each vessel. A careful comparison of these two columns of freeboards, viz., those as marked by the disc centre and those required by Mr. Martell's tables, will afford an opportunity to all who honour them with an inspection, of drawing their own conclusions as to how far the present law has checked over-loading. My own conclusion is that it has not, and that there are many instances here given of vessels marked with a side which a few years ago would have been thought by practical men inadequate for safety.

To discuss all the causes which have brought this about, or the custom—not to say the law—that appears to sanction it, might tend to wake up an agitation that would do well for a time to rest. I cannot, however, refrain from expressing an opinion that the present accepted principle of freeboard, which implies that a percentage of 25 to 30 per cent. spare buoyancy alone suffices for safety, is, when applied to certain types of vessels, likely to favour deep loading. Beyond doubt, spare buoyancy is an essential element of seaworthiness. It secures a certain specific gravity or relative lightness of ship, and in proportion to its amount prolongs the life of a foundering ship. But its power to lift, or even to sustain, may be neutralised when a long vessel is unequally borne amongst waves, and her movements principally governed by their vertical and horizontal impulse. Practical experience appears to suggest that the primary object in apportioning freeboard should be to secure a height of platform above the water sufficient to prevent the decks being swept by heavy seas, for when this occurs it frequently is the commencement of a chapter of accidents which ends in disablement and abandonment. I am well aware there are vessels of good form that may be loaded down very near the water, and if carefully stowed and judiciously handled will pass through the heaviest seas without shipping much water. This would not, however, be the case with, say, the ordinary long flush-

decked cargo steamers of eight beams' length and over, loaded with heavy cargoes, the weights of which are most unequally distributed, and having centres of gravity and metacentres nobody knows where.

In making these remarks I do not forget that the tables of freeboards here attached furnish evidence of vessels loading to a much less side than that represented by a spare buoyancy of 25 per cent., and that it may fairly be inferred the greater number of them succeed in making their passages. Those who understand the subject will know that experience does not justify the inference, that because a vessel is deep she must be lost. By bringing steamers well up, head to sea, and going dead slow with the engines, they may be nursed through very dirty weather, but there is always the risk of an insufficient freeboard bringing about disaster, and contributing to their loss.

WOOD VESSELS (First Class).

Number.	Description of Vessel.	Registered				Freeboard				Coefficient of Fineness.
		Under Deck Tonnage.	Length.	Breadth.	Depth.	As marked by Disc Centre.		As required by Mr. Martell's Tables.		
			ft.	ft.	ft.	ft.	in.	ft.	in.	
1	Poop 66′, Forecastle 40′	1159	211·0	35·3	22·0	5	6	5	11¾	·70
2	,, 61′, ,, 35′	1033	184·4	31·7	21·1	4	3	5	2	·62
3	Raised Quarter-deck 58′, Forecastle 15′ ...	853	186·5	33·5	22·0	4	5	5	5¾	·62
4	Flush with Deck-houses	839	180·2	32·3	22·5	4	1½	5	9¼	·64
5	Poop 48′, Forecastle 34′	826	162·9	34·6	22·5	5	8	5	11	·66
6	,, 48′, ,, 37′	771	180·4	33·6	20·8	4	0	5	0¼	·61
7	Raised Quarter-deck 59′, Forecastle, 23′ ...	744	186·4	32·4	20·8	4	9	4	11	·59
8	,, ,, 47′	738	186·0	30·5	20·8	4	4½	5	1¾	·63
9	Poop 68′	721	175·0	32·0	21·0	4	8	5	1¼	·61
10	Flush with Deck-houses	699	170·0	30·3	19·3	4	6½	5	0¾	·70
11	Poop 45′, Forecastle 34′	698	182·0	31·0	19·0	4	0	4	8¾	·65
12	,, 50′, ,, 29′	642	151·0	31·8	20·7	5	0	5	2½	·64
13	Flush with Deck-houses	489	148·2	29·6	17·7	3	1	4	3½	·63
14	Break-deck 49′...	476	138·0	29·0	18·7	4	9¼	4	6½	·63
15	,, 15′, Forecastle 17′	469	141·0	29·5	17·9	4	5	4	3½	·63
16	Raised Quarter-deck 30′, Forecastle 30′ ...	468	146·3	29·0	18·4	3	10½	4	3¾	·60
17	Break 15 tons	449	140·9	27·0	18·2	3	7	4	6¼	·65
18	Poop 25′	438	145·6	30·1	16·5	3	0	4	2¾	·70
19	Raised Quarter-deck 30′	406	137·0	27·9	17·2	3	2½	4	1	·62
20	,, ,, 34′	401	136·1	27·6	17·1	4	0	4	0¼	·62
21	,, ,, 26′	400	131·5	26·5	17·9	3	9	4	4¾	·64
22	,, ,, —	381	129·0	28·0	17·2	3	7	4	0½	·61
23	,, ,, 30′	378	126·0	26·6	16·9	2	1	4	2½	·66
24	,, ,, 27′	378	129·0	28·1	17·3	3	7	3	11¾	·60
25	,, ,, 24′	362	133·2	25·1	16·3	2	0	4	0¼	·67
26	,, ,, 36′	361	130·5	27·3	17·0	3	4	3	11	·60
27	,, ,, 32 tons	350	130·0	27·0	16·0	3	5	3	9	·62
28	,, ,, 44′	340	148·5	29·9	12·3	2	11	2	8½	·62
29	Flush...	325	130·6	27·2	14·1	2	11	3	3½	·65
30	Break-deck 18 tons	310	134·3	26·3	14·3	2	7½	3	2¾	·61
31	Flush...	305	111·1	27·5	15·9	3	7	3	9½	·63
32	Break-deck 11 tons	286	111·4	26·5	16·2	2	9	3	11¼	·65
33	Flush	304	113·4	25·7	15·0	3	2	3	9	·69
34	,, ...	284	113·4	26·1	15·7	3	6	3	7½	·61
35	Raised Quarter-deck 42′, Forecastle 17′ ...	281	129·3	27·5	13·0	1	9	2	10¼	·60
36	,, ,, 36′	270	124·3	25·2	15·2	2	9	3	4	·57
37	,, ,, 33′	266	118·6	23·8	15·0	2	7	3	6	·63
38	,, ,, 9 tons	266	120·0	25·0	14·8	3	8	3	3¾	·60
39	Flush...	264	123·9	26·6	12·9	2	4½	2	11	·62
40	Half Poop 29′	260	129·2	27·0	12·9	2	4½	2	9½	·58
41	Flush...	258	119·6	24·0	14·4	2	1	3	3¾	·62
42	Raised Quarter-deck 26′	258	125·0	25·1	13·9	2	2	3	1	·59
43	,, ,, 9 tons	255	115·5	24·6	14·6	2	6	3	3½	·61
44	Flush...	229	114·3	24·8	12·9	2	4	2	11¼	·63
45	,, ...	216	112·0	22·7	13·7	2	2½	3	1¼	·62
46	Raised Quarter-deck 21′	207	113·6	22·8	12·5	2	1	2	9¾	·64
47	Break-deck 18 tons	207	118·2	24·5	12·9	2	7½	3	2¾	·61
48	Flush...	206	110·6	25·1	12·3	2	6	2	7¾	·60
49	Break-deck 7 tons	206	111·6	23·9	12·8	2	3	2	9½	·60

WOOD VESSELS (First Class)—*continued.*

Number.	Description of Vessel.	Registered				Freeboard		Co-efficient of Finess.
		Under Deck Tonnage.	Length.	Breadth	Depth.	As marked by Disc Centre.	As required by Mr. Martell's Tables.	
			ft.	ft.	ft.	ft. in.	ft. in.	
50	Flush...	188	104·2	24·1	12·8	2 0	2 8¾	·58
51	,, ...	187	97·0	20·2	13·1	2 2½	3 3¼	·73
52	,, ...	186	103·2	23·6	12·9	1 10	2 9¾	·59
53	,, ...	183	105·6	24·5	12·9	2 4¾	2 9½	·62
54	,, ...	180	102·7	22·7	12·5	2 4	2 9¼	·62
55	,, ...	177	102·0	23·5	12·9	2 4	2 9¼	·57
56	Raised Quarter-deck 27′	176	104·6	22·1	12·5	1 10	2 11¾	·70
57	Flush...	172	113·4	20·5	12·0	1 8	2 7¾	·62
58	,, ...	171	104·2	23·6	12·7	2 0	2 8	·55
59	Raised Quarter-deck 21′	165	81·3	20·8	14·2	2 5	3 5½	·68
60	Flush...	156	98·4	23·3	11·2	1 8	2 4¾	·60
61	,, ...	156	99·4	22·2	12·1	1 10	2 6½	·58
62	,, ...	150	100·0	22·4	12·0	1 10	2 6	·56
63	,, ...	96	81·4	16·6	10·4	1 6	2 3½	·68

WOOD VESSELS (Colonial Built, Soft Wood).

64	Flush with Deck Houses	555	142·0	30·5	18·0	3 8	4 9	·71
65	,, ,, ...	490	138·5	26·7	17·8	4 0½	4 9¾	·74
66	,, ,, ...	298	118·0	27·4	14·9	3 3½	3 5¼	·61
67	,, ,, ...	293	120·0	28·0	14·9	2 6	3 4¼	·59
68	Poop 27 tons	281	132·0	29·0	12·8	2 1	2 8	·57
69	Flush with Deck Houses	250	112·3	25·4	13·9	2 10½	3 8	·76
70	,, ,, ...	222	102·0	24·1	12·7	2 10	3 0¼	·71
71	,, ,, ...	150	96·0	23·6	11·3	1 10	2 4½	·59

COMPOSITE VESSELS.

72	Raised Quarter-deck 21 tons	399	136·9	27·7	16·2	3 8	3 7¼	·59
73	Flush...	200	110·0	23·0	11·8	2 1	2 7½	·67

LEE-BOARD WOOD BARGES.

74	Flush...	116·0	100·0	23·2	8·0	0 10	1 6¼	·62
75	,, ...	111·0	96·8	23·0	8·0	0 10	1 6¼	·62

IRON VESSELS.

76	Poop 40′, Forecastle 37′, 2 decks	1392	246·3	38·3	22·8	4 9½	5 8	·65
77	,, 35′, 2 decks	1379	224·3	37·8	23·5	4 10	6 1¾	·69
78	Poop 50′ ,, 30′, 2 decks	1366	237·4	38·4	22·5	4 4	5 1	·67
79	,, 36′ ,, 26′, 2 decks	1213	227·6	37·0	22·5	4 6	5 6¼	·64
80	,, 45′ ,, 32′, 2 decks	1192	229·3	36·8	22·3	3 5	5 4½	·63
81	,, 56′ ,, 39′, 2 decks	1095	213·8	34·8	22·1	4 6	5 5¾	·66

IRON VESSELS—*continued.*

Number.	Description of Vessel.	Registered				Freeboard		Coefficient of Fineness
		Under Deck Tonnage.	Length.	Breadth.	Depth.	As marked by Disc Centre.	As required by Mr. Martell's Tables.	
			ft.	ft.	ft.	ft in.	ft. in.	
82	Poop 38′, Forecastle 35′, 2 decks	1056	226·0	35·3	20 5	3 11	4 10¾	·64
83	,, 30′, 2 decks	1032	222·0	35·3	21·0	4 5	4 11½	·63
84	Poop 37′ ,, 32′, 2 decks	943	217·0	33·6	20·0	3 9½	4 10	·65
85	,, 53′ ,, 35′, 2 decks	873	216·6	35·2	20·5	4 3½	4 6½	·56
86	Raised Quarter-deck 39′, Forecastle 17′ ...	663	177·4	30·7	18·5	3 5	4 5	·66
87	,, ,, 36′	573	167·6	28·6	17·7	3 3	4 3¾	·68
88	,, ,, 42′	372	158·0	25·2	15·5	3 2½	3 4½	·60
89	,, ,, 33′	241	140·6	21·7	13·9	2 7	2 11¼	·57
90	... ...	163	104·2	21·8	11·6	1 10	2 5	·62

IRON STEAM VESSELS.

91	Two Decks & Shade Deck in part, 4 tr. beams	4384	360·4	52·3	34·7	7 4	...	·75
92	Flush with Houses	3816	384·0	41·3	35·3	12 6	...	·68
93	,, ,, ...	3709	377·6	42·4	33·0	11 6	...	·70
94	Poop 96′, Forecastle 52′, 2 decks, 3 tr. beams	3158	389·5	42·1	28·9	5 6	8 10	·67
95	,, 96′, ,, 52′, 2 decks, 3 tr. beams	3153	389·0	42·1	28·8	6 0	8 9	·67
96	Flush with Houses and Forecastle	2931	379·8	38·3	28·5	7 4	8 4½	·70
97	,, ,, ,, 22 tons ...	2923	380·0	38 3	28·3	7 4	8 4½	·70
98	,, ,, 3 decks	2558	350·4	36.4	28·1	8 3	8 9½	·71
99	,, ,, 2 decks, 3 tier beams ...	2198	316·4	35 8	25·9	5 11	7 8½	·75
100	,, ,, 3 decks	2095	321·8	36·4	26·4	6 8	7 4¾	·68
101	,, ,, 2 decks, 3 tier beams ...	2085	331·2	34·4	24·4	5 7	7 1¼	·75
102	,, ,, 2 decks, 3 tier beams ...	2068	330·0	34·9	24·4	5 0	7 0	·74
103	Poop 68′, Forecastle 43′, 2 decks, 3 tr. beams	1931	302·2	35·2	25·0	4 8	6 2	·72
104	Flush with Houses, 2 decks, 3 tier beams ...	1925	300·3	34·7	25·2	5 4	7 4	·73
105	,, ,, 2 decks, 3 tier beams ...	1918	312·0	36·0	25·8	5 6	6 9	·66
106	,, ,, Forecastle 21′, 3 decks ...	1828	301·0	33·2	25·0	6 5	7 1	·73
107	,, ,, 2 decks, 3 tier beams ...	1801	290·2	34·1	25·0	5 8	6 10½	·72
108	Flush Break 58′, Forecastle 40′	1797	309·8	32·9	24·0	7 8	6 8½	·74
109	Flush with Houses, 3 decks	1762	298·0	33·2	25·4	7 7	7 0¾	·70
110	,, ,, 3 decks	1723	291·5	33·2	24·6	6 3½	6 8	·72
111	,, ,, 2 decks, 3 tier beams ...	1660	270·7	33·0	24·5	5 0	6 7½	·76
112	,, ,, 2 decks, 3 tier beams ...	1653	290 0	33·7	23·9	6 1½	6 3½	·71
113	,, ,, ...	1642	276·3	33·1	23·9	6 6	6 6¼	·75
114	Poop 33′, Forecastle 32′, 2 decks 3 tr. beams	1559	258 0	34·2	23·3	4 1	5 2½	·76
115	Flush with Houses, Forecastle 21′, 2 decks, 3 tier beams...	1548	259·8	32 6	23·5	4 9½	5 11½	73
116	Flush with Houses	1513	280·2	31·6	23·7	5 7	6 0	·72
117	Poop 40′, Forecastle 33′, 3 decks	1420	257·3	32·0	23·6	4 9½	5 11½	·73
118	Flush with raised ends, 2 decks	1394	249·0	33·3	23·8	5 1	5 10	·70
119	Flush, 2 decks, 3 tier beams	1392	244·7	32·3	24·0	7 4	6 2½	·73
120	Poop 24′, Forecastle 31′, 2 decks	1384	240·0	33·0	24·0	4 0	6 8½	·73
121	Flush with Houses, Forecastle 23′, 3 decks...	1294	241·8	32·1	23·2	5 9½	5 9½	·72
122	Poop 26′, Forecastle 30′	1294	250·6	32·7	20·3	3 0	5 0	·78
123	Flush, 2 decks, 3 tier beams	1261	241·7	32·3	22·7	5 4	5 6½	·71
124	,, 2 decks, 3 tier beams	1173	240·3	31·2	21·8	5 6	5 2½	·72
125	Bridge deck 56′, 2 decks...	1166	230·3	32·2	19·9	2 11	4 11½	·79

IRON STEAM VESSELS—*continued.*

Number.	Description of Steam Vessel.	Registered				⊖ Freeboard		Coefficient of Finess.
		Under Deck Tonnage.	Length.	Breadth.	Depth.	As marked by Disc Centre.	As required by Mr. Martell's Tables.	
			ft.	ft.	ft.	ft. in.	ft. in.	
126	Flush, 2 decks, 3 tier beams	1109	230·7	31·2	22·0	5 3	5 1¾	·70
127	,, 2 decks, 3 tier beams	1079	221·5	30·7	21·4	4 6	4 10¼	.74
128	Break deck 57 tons	840	210·0	28·7	18·9	2 8	4 3¼	·74
129	Poop 39′, Forecastle 31′	694	203·9	28·9	16·9	2 3½	3 5½	·70
130	Raised Quarter-deck 18 tons	652	200·0	28·0	15·9	2 1	3 2½	·71
131	Long Poop and Forecastle	2095	363·0	38·5	23·7	6 0	4 11	·63
132	,, ,, ...	1264	267·0	33·3	18·9	1 10	3 8½	·75
133	,, ,, ...	1258	258·5	34·1	19·7	2 5	3 10	.72
134	,, ,, ...	1246	264·5	33·4	18·8	2 2	3 7½	.75
135	,, ,, ...	1090	243·2	32·8	18·0	1 9	3 5	·76
136	,, ,, ...	975	240·2	32·0	16·7	1 0	3 1	·76
137	,, ,, ...	974	245·0	32·0	17·9	3 0	3 3	.71
138	,, ,, ...	965	248·0	32 5	17·3	3 3½	3 0	·69
139	,, ,, ...	939	245·0	31·0	17·3	2 0½	3 1	·72
140	,, ,, ...	904	231·5	30·3	17·5	2 6	3 3½	·73
141	,, ,, ...	888	230·0	31·2	17·6	2 10	3 0	·70
142	,, ,, ...	870	230·4	31·1	16 6	3 4	2 11	·73
143	,, ,, ...	846	233·0	29·1	16·6	2 8	3 0¼	·75
144	,, ,, ...	843	229·8	30·0	16·9	1 0	3 0	·72
145	,, ,, ...	841	230·6	32·2	16·2	2 3	2 9½	·70
146	,, ,, ...	830	224·4	30·1	17·3	2 2½	3 1	·71
147	,, ,, ...	825	230·0	30·2	16·2	2 6	2 10¾	·73
148	,, ,, ...	805	221·4	30·4	17·6	2 4	3 0¼	·75
149	,, ,, ...	749	209·2	29·1	16·7	2 4	2 11	·70
150	,, ,, ...	722	237·4	28·1	16·0	2 2	3 8½	·68
151	,, ,, ...	651	208·1	28·2	15·9	1 11	2 8½	·70
152	Long raised Quarter-deck and Forecastle ...	1427	258·0	34·0	22·1	3 6	4 10½	·74
153	,, ,, ,, ...	1116	240·2	33·0	17·9	2 6	3 8½	·78
154	,, ,, ,, ...	1101	246·6	32·2	18·2	1 10	3 8¾	·76
155	,, ,, ,, ...	902	231·3	32·2	18·0	3 8	3 2½	·67
156	,, ,, ,, ...	827	225·3	30·1	16·7	1 8	3 2	·73
157	,, ,, ,, ...	824	225·0	29·7	16·7	1 6	3 2	·74
158	,, ,, ,, ...	808	224·2	28·9	16·1	2 6	3 5¼	·77
159	,, ,, ,, ...	807	224·4	30·0	16·9	1 8	3 2	·71
160	,, ,, ,, ...	803	220·0	30·0	16·1	1 6½	3 0¾	·76
161	,, ,, ,, ...	646	210·8	28·3	17·0	2 0	2 10	·64
162	,, ,, ,, ...	643	194·9	28·6	15·9	1 5	2 11½	·73
163	Spar decked, of good structural strength above water, for cargo	1261	299 5	31·8	$\frac{25\cdot8}{18\cdot8}$	0 6	3 2½	·67
164	,, ,, ,, ,,	912	255·0	32·2	$\frac{17\cdot6}{24\cdot8}$	0 0	2 9½	·63
165	,, ,, ,, ,,	899	254·0	32·2	$\frac{17\cdot3}{24\cdot9}$	Maindeck1ft. under water.	2 9	·64
166	,, ,, ,, ,,	737	246·8	31·6	$\frac{16\cdot6}{23\cdot7}$	Maindeck2ft. under water.	2 4	·57
167	,, ,, ,, ,,	695	230 7	31·2	21·8	5 6	From upper deck.	·70
168	,, ,, ,, ,,	652	227·5	29·4	$\frac{15\cdot7}{23\cdot0}$	M.dk.1ft.4in. under water.	2 4	·62
169	Awning decked and Poop 54 tons	563	234·0	27·6	13·7	2 0	1 2½	·64
170	Auxiliary Screw	1949	286·4	39·2	26·4	6 0	...	·66
171	,, ,, ...	1787	279·7	38·1	24·0	3 0	...	·70
172	,, ,, Poop 52′, Forecastle 40′ ...	1168	249 7	32·8	22·3	4 3	...	·64
173	,, ,, ,, 144 tons, ,, 23′ ...	1023	209·2	34·3	22·0	4 1	...	·64

DISCUSSION.

Mr. C. W. Merrifield, F.R.S.: My Lord and gentlemen, I think the sole interest of this paper consists in the tables at the end, which afford a comparison between the load-line actually placed on vessels under the name of "Plimsoll's mark" and Mr. Martell's tables. The first column, I think, is of by far the greatest interest. The column including Mr. Martell's tables certainly affords us a not altogether useless means of comparison. At the same time, with regard to the mark itself, I do not think we have much to learn from it. That mark was simply a sacrifice to Moloch. My private opinion is that the best place for it would be the maintop. I do not think it has added to the security of vessels, but the actual statement of where they are placed on the average is not without interest. In any other way I cannot conceive the subject to be of the least practical value. I should at the same time say that we are extremely obliged to the author for the trouble which he has taken in giving us this comparison.

ON THE DOUBLE POWER FLOATING DOCK AND THE HYDRAULIC GRID.

By Latimer Clark, Esq., C.E.

[Read at the Twentieth Session of the Institution of Naval Architects, 4th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

In 1876 I had the honour of reading a paper before the Institution of Naval Architects on a new form of dock, called the Depositing Dock, the first of which was then about to be constructed for the Russian Government at Nicolaieff; and last session Captain E. E. Goulaeff, R.I.N., gave a detailed description of this dock after its completion, showing that our most sanguine expectations had been fully realised. This dock, from its requiring fixed stages on which to deposit the vessels when raised, is more especially suited for quiet harbours or rivers in which there is no considerable current or rise and fall of tide, and where the commerce is so great as to require the docking of several vessels at the same time.

The numerous inquiries we have received from other places where these conditions are not obtainable, together with our practical experience of the varying conditions under which vessels have to be docked, have led us to give the question prolonged study, and to design special forms of docks for meeting such cases.

Two of these new forms appear to us especially worthy of the attention of this Institution. These are called the Double Power Floating Dock and the Hydraulic Grid; the first being detached and requiring no foundations, and the second being a fixture, adapted for use on the foreshores of tidal rivers.

The Double Power Floating Dock.

This dock has been so named in consequence of its utilising the buoyancy of its sides in combination with that of the body or pontoon part of the dock. The necessary result of this arrangement is, that the quantity of ironwork in a dock of any given power, is very much less than that in any other form of floating dock at present known.

In all existing floating docks the sides, which are usually the heaviest portion of the structure, are out of use when the vessel has been raised, and not only are they useless, but the dock has to be made by so much the larger and more buoyant in order to support their unserviceable weight; the sides are only required when the dock is being submerged in

order to receive or discharge a vessel, and it is obvious that if means can be devised for getting rid of their weight, the pontoon may be made by so much the smaller, and if in addition to this their enormous buoyant power can be brought into play, the size of the body of the dock can be still further reduced; or if there be no reduction made in the pontoon, and the sides be made to give buoyancy instead of adding weight, the lifting power of the dock will be by so much increased: in fact, it is in practice fully doubled.

Both these advantages are obtained by so constructing the sides of the dock that they can slide freely up and down in grooves, and be readily secured in any desired position. The *modus operandi* is as follows:—Before lowering the dock to receive a vessel, the sides are fixed in such a position that the dock is in the ordinary form, and water being admitted in the usual manner it sinks to the required level.

The vessel having been admitted, and slightly lifted and shored in the ordinary way, water is pumped out of the pontoon of the dock only, until the whole of its buoyancy has been brought into action. At this stage of the operation, the vessel being half-raised, the water in the two sides is so regulated that they are successively made just self-supporting, and neither add to nor detract from the power of the dock. There are ready means for ascertaining this point. In order to complete the operation and bring the whole power of the dock into action, one of the sides is then unpinned, and water admitted into it until it descends to its lowest level, the top being allowed a freeboard of about five feet; it is then rigidly pinned and secured in its new position, and the other side is lowered and secured in a similar manner. It is now only necessary to pump water out of these sides to add their submerged buoyancy to that of the dock, and so complete the operation.

Fig. 1 (Plate XXI.) shows a plan of the dock with a vessel outlined upon it, and Fig. 2 shows an end elevation. B B B B are four corner towers rigidly attached to the body of the dock, and of such a height that they have a freeboard of about five feet when the dock is at its lowest level; they are constructed with air-tight compartments, and their object is to add to the stability while the dock is being raised and lowered, and to make the dock unsinkable even if all the valves were purposely left open. A A and A A are the sliding parts of the sides; they extend along the greater portion of the length of the dock, and contain the engines and pumps. C is the pontoon or body of the dock, which, it will be observed, is provided with pointed ends, in order that the buoyancy may be principally given under the heaviest part of the vessel, and to enable the dock to be towed easily.

Fig. 3 (Plate XXI.) shows a side elevation with the vessel raised, and with the sliding sides A A lowered so as to utilise their buoyancy.

At first sight the arrangement of the sliding sides and the means of securing them in any required position might appear to present some difficulty, but a reference to the figures will show that no real difficulty has to be encountered.

The sides are guided in their movement by a great number of steel slides attached to strong vertical side shoring frames, which are built on the body of the dock as indicated in Figs. 7 and 9 (Plate XX.); these frames serve the double purpose of shoring the vessel and acting as guides for the moveable sides: the form of these slides is shown in greater detail

and full size in Figs. 4 and 5 (Plate XXI.); Fig. 4 being an elevation and Fig. 5 a section. DD are two vertical slides, resembling railway bars, one of which is riveted to the shoring frames of the dock, and the other to the sliding side. These vertical slides are connected by two box slides of steel, marked E and F. In Fig. 4, E is the upper box slide, and F the lower one; they are joined by a connecting-rod G, so that both move together. The upper one is pierced with holes for receiving steel pins, by which they can be secured at any required level; these pins are shown in section in Fig. 5.

The upper box slide E is first secured by two pins to the shoring frame slide, which is on the right-hand side of the figure; the side of the dock is now free to rise and fall, sliding through the box slides. As soon as the side rises to the desired level, pins are inserted through the slotted holes in the box slide E, and as soon as these pins take their bearing, the holes will also be fair to receive other pins, which will keep the side rigidly in position. The dock and sliding sides are now securely attached.

These slides occur every six feet on each side of the dock.

The engines are two in number, fixed in engine-rooms in the sliding sides. Each engine drives two centrifugal pumps, one pump controlling each side, and one pump each lateral half of the body of the dock—that is to say, one pump in each side, and two in the body of the dock. The power is communicated from the sides to the pumps in the body of the dock by means of bevelled wheels, one of which is fixed, and the other slides up and down on a grooved vertical shaft fixed on the pontoon and connected with the pumps below. In very large docks there might be an advantage in placing the engines in the four corner towers.

It might appear at first sight that in unfastening one of the sides of the dock in order to raise or lower it, the dock would be deprived of much of its stability, and this is undoubtedly the fact. A little consideration will, however, show that there is, nevertheless, at all times more than abundant stability.

As regards the vessel itself, it must be remembered that being only half out of water it retains nearly as much stability as when afloat, and any movement towards either side causes nearly the same displacement, and brings into action nearly the same righting power, as if she were at her ordinary light load line.

The stability of the vessel taken *per se* is, however, a matter of little importance. The real stability lies in the fact that the vessel and the remaining fixed side of the dock are practically in rigid connection, and form one floating structure with a width of beam probably twice as great as that of the vessel itself, and any listing movement is immediately met and resisted either by the submerging of the whole area of the vessel, or the whole area of the side of the dock, or by both, the centre of gravity lying somewhere between the vessel and the fixed side of the dock. This is well illustrated in the case of the Nicolaieff Depositing Dock. It is well known, however, that there is a point where the vessel adds no stability to a floating dock, that is when her bilges and keel are just rising out of the water; but long before this position is approached both sides of the dock have been successively lowered and rigidly secured, and the stability is that due to the two sides in combination.

It may be well to remark that in the majority of cases—that is to say, with moderate-sized vessels—it would be unnecessary to use the sliding sides, but the dock would have sufficient power to be used as an ordinary floating dock.

It will be seen that the longitudinal strength of this dock is very great indeed; the sliding sides adding their strength to that of the bottom.

We have hitherto spoken of the dock as having four fixed corner towers, in order to give extra stability and secure a constant surplus floating power at all times; but it may be of interest to those who carefully study questions of this nature, to observe that there is no real necessity for having any portion of the sides permanently fixed, and some economy and advantages may be obtained by dispensing with these fixed towers, and constructing the dock in the form shown in Figs. 6, 7, 8, 9, and 10 (Plate XX.) In this form of the dock, the sides can only be raised or lowered under two conditions: first, when there is a vessel on the dock giving stability in the manner already described, or secondly, when the pontoon or body of the dock is raised and the deck above water. In either of these positions the sides may safely be raised or lowered successively, and fixed in any desired position.

By dispensing with the corner towers we not only save their cost, but save also the cost of the floating power required to support their weight, and utilise both as effective buoyancy.

We have not hitherto mentioned one of the most invaluable features of this form of dock, viz., that it is perfectly and readily self-docking, without requiring to be taken apart or removed from its moorings, and every part of the dock, both inside and out, is easily accessible for cleaning and painting; even the underside of the body of the dock can in less than two hours be raised on an even keel several feet clear of the water, and examined with the greatest facility. This is done by lowering the two sides until their decks are level with the deck of the pontoon; after being secured in this position they are pumped out, and raise the body of the dock several feet clear of the water, as shown in Figs. 7 and 8. The under portions of the sides can be got at with equal ease. It will be seen by Figs. 9 and 10 that when the sides have been secured in their highest position, and the dock is pumped out, they are raised entirely out of the water, and are everywhere accessible.

We may be permitted to lay great stress on the importance of this feature, as it is well known that the necessity of ready accessibility for cleaning and maintenance is one of the greatest difficulties of all existing floating docks. With the exercise of proper care and the great facilities of access possessed by this dock, it is almost impossible to assign any limit to its durability.

The Hydraulic Grid.

The second form of dock which we desire to describe is called the Hydraulic Grid, and bears considerable resemblance to the Hydraulic Lift Dock of Mr. Edwin Clark.

That dock, as is well known, is constructed with two rows of hydraulic presses and rams, between which hang suspended a number of transverse girders forming a gridiron, which supports a pontoon upon which the vessel when raised is ultimately floated. These docks

perform their functions admirably, but they are chiefly adapted for comparatively deep water, and for places where there is no very large rise and fall of tide.

The Hydraulic Grid is, on the contrary, only suited for places where the rise and fall of tide is great, the vessel being lifted only at high water, and consequently the greater part of the work being performed by the tide itself.

As in the Hydraulic Docks of Mr. Edwin Clark, the vessel is raised by hydraulic presses and rams, but here the resemblance ceases. The presses are placed directly beneath the vessel to be raised, and this circumstance enables us to dispense with the lofty double row of guiding columns, the numerous cross girders which have to be of great strength and weight, and the yokes and chains by which they depend from the tops of the rams. The pontoon is also dispensed with, and its place is supplied by a simple grid of wrought iron, upon which the vessel is shored and raised. By thus dispensing with so many parts which are necessary in ordinary cases, but are not required in this form of dock, the weight of the apparatus to be lifted is reduced 75 per cent., and consequently the hydraulic presses are proportionally reduced in number or size; and lastly, since the lifting operation is commenced only at high water, the length of the presses is diminished by about one-half. From all these causes the economy in cost of construction is very great indeed.

In favourable positions, such for example as on the foreshores of the Thames or the Mersey, hydraulic grids may be constructed at a cost of five pounds per ton of dead weight to be docked, while as compared with patent slips they have the advantage of occupying very little space, and of raising vessels on an even keel without the slightest strain.

Fig. 11 (Plate XXI.) is an end elevation of the grid, from which it will at once be seen that the rams are more or less inclined, so as to cause the grid, as it rises and falls, to bear steadily against the quay wall. Fig. 12 is a side elevation, and Fig. 13 represents it in plan. The large columns A A are fender columns carrying a gangway; they also serve as vertical guides. The presses are marked B B, and they are arranged in the usual manner in three groups, supporting the vessel triangularly. The presses are ranged principally under the keel of the vessel, a few only being placed under the bilges, sufficient to give transverse stability. The grid itself is a rigid wrought iron skeleton structure of plate and angle irons, carrying at the middle portion a number of side shoring frames for retaining the vessel in position.

The grid, when raised, may be either supported on the rams, or in some cases on strong legs or supports, which are hinged at the top and are allowed to fall down into a vertical position when the grid is raised; it is then allowed to rest upon them, and the rams may then descend into their presses until again required; in this position they are not exposed to rust. In tidal rivers where the ground is uncovered at low tide the presses can be sunk into the ground and concreted with great facility, and the leathers are readily accessible; in cases where the presses cannot be driven or screwed in a true position, an outside cylinder is sunk and the press dropped into it.

The grid is furnished with automatic safety valves, so arranged that if a leather should burst in one group, and the grid begin to get out of level, the other groups would automatically lower themselves by the slight tipping movement of the grid itself. Very small

engine power is required, as it is only necessary to raise the vessel while the water is falling, and before the next tide, and as the lifting operation commences at high water the distance it has to be raised is a minimum.

Having thus described these two forms of dock, we would remark that while the Double Power Dock is suited for deep water, rapid rivers, and exposed positions, and requires no foundations, the Hydraulic Grid is adapted for the foreshores of rivers and harbours, when there is a great rise and fall of tide, so that every possible set of conditions is met either by one of these two docks or by the Depositing Dock first alluded to. One of the principal merits of these docks arises from the fact that by great attention to detail they have been so designed that their cost in all cases is reduced greatly below that of docks as hitherto constructed.

There are numberless places where docks have long been urgently required, but where their great expense has hitherto been a barrier to their introduction. That obstacle can now be scarcely said to exist, and from the small cost at which these two forms of dock can be constructed and worked, there can be no doubt that they will, when known, come into extensive use, and prove a real boon to commerce.

DISCUSSION.

Mr. E. J. Harland: I should like to ask the writer of this paper whether such a plan of a dock has been constructed, and has been in practical use?

Mr. J. Scott Russell, F.R.S.: We all know that Mr. Clark and his friends have already constructed a multitude of similar, but unsimilar structures, in which a great many of the principles involved in this have been carried out, and in this valuable paper he seems to me to have given us the result of a multitude of years of most extensive experience in work of this nature, all of which, I believe, has been successfully carried out, and, therefore, he is merely giving us now some modifications of things which, not the same, though in some respects similar, have been perfectly successful, which we are all acquainted with. Therefore we must all be grateful for the compliment he pays us in laying before us certain modifications of this, even if they have not already been carried into execution.

Admiral J. H. Selwyn: I think the Navy ought to express their obligations to gentlemen like Mr. Latimer Clark, who have shown us that it is possible in distant countries to have docks established, which never could be done by the expensive excavation and building which was formerly our only resort. We know that they will be more than ever needed now that we have come to understand the disease of surface-friction, and the probability that the new warfare will be conducted very largely under water. Consequently on his discovery our ships may hope to get repairs on their distant stations, without being taken from those points where they are so urgently required. It seems to me that every advance that Mr. Latimer Clark has made has been in the direction of what is of special value to the Navy, diminishing the expense at which these repairs can be effected, and adding to the security of the kingdom. I have seen a great deal of floating docks in foreign countries, but they have generally been very cumbrous structures, and I am very sorry to say a great many of them have found their end by sinking to the bottom of the harbour in which they laid, remaining there and

refusing to do any more work afterwards. That was in the early days of floating docks, but I think Mr. Latimer Clark has shown, especially in the Russian ports, the enormous value of a rapid means of docking vessels as they come in. I am afraid some of the disasters to the Turks are clearly traceable to the fact that Mr. Latimer Clark aided the Russians in cleaning their ships' bottoms.

Mr. C. W. Merrifield, F.R.S.: My Lord and gentlemen, I agree in the extreme value in a national point of view of any easy means of constructing docks in our different Colonies. One of the greatest dangers, I take it, of our present Navy is the necessity under which it lies of returning to England, with the only alternative of making a very few of the principal ports in our Colonies, before they can refit for any useful purpose. I think Mr. Latimer Clark, by placing cheap docks within our reach for a much larger number of ports than we could have ventured to apply them to, is the author of a real national benefit. I certainly wish him success, and I hope he has attained it.

The President: Perhaps, Mr. Clark, in your reply, you will explain Fig. 11, Plate XXI. I do not understand it.

Mr. Latimer Clark: I will reply to the questions as put. In answer to the first question, I may say that none of these docks have yet been constructed—in fact, they were only invented during last year. I should explain to my Lord that Fig. 11 represents the hydraulic grid with a vessel upon it. The left-hand wall is supposed to be a quay-wall, and the right-hand pillar is merely a fender column, serving as a defence and guide. The three perpendicular lines in the centre represent three rows of hydraulic presses, of which the centre one is a row extending the whole length of the keel of the vessel. The other two are chiefly under her bilges. The horizontal line which rests on those three columns is the body of the grid, and the two perpendicular portions on the right and left-hand sides supporting the vessel are of course the shoring-frames. The vessel itself is seen in section.

The President: I think that closes our business for this evening.

PROCEEDINGS ON APRIL 5TH.

On Saturday, April 5th, at noon, Lord Hampton in the chair, the Secretary read the names of the Officers of the Institution, who were unanimously re-elected. Mr. John Corry was elected Auditor, in room of the late Mr. R. Carlyle, deceased.

ON THE MOST POWERFUL IRONCLAD.

By Rear-Admiral J. H. Selwyn.

[Read at the Twentieth Session of the Institution of Naval Architects, 5th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

The most Powerful Ironclad should have :—

The most stable gun platform combined with the greatest stability.
The most powerful guns in the greatest number.
The greatest speed for the longest time.
The thickest armour of the most durable character.
The least exposure of vertical height to shot.
The least liability to be fatally rammed.
The least liability to fatal torpedo attack.
The least draught of water with which these advantages can be secured.

The "Britannia" (See Plate XXII.)

Length on water-line	350 feet.
,, deck	370 ,,
Beam on water-line	200 ,,
,, deck	220 ,,
Draught forward	18 ,,
,, aft	13 ,,
Height of upper deck above keel (when deck is raised)	36 ,,
Area of midship section	2,333 square feet.
Wetted area	60,000 ,,

Weight of hull (steel) unarmoured	2,600 tons.
Displacement	14,767 ,,
Total weight of armour	4,775 ,,
Thickness of armour round edge of gunwale	30 inches.
,, ,, ,, gun-pits	30 ,,
Depth of armour round gun-pits	48 ,,
Freeboard...	8 ,,

Armament, twenty 80-ton guns, or eight 100-ton and eight 80-ton. All this weight of metal capable of being fired in any direction in five minutes—nearly 13 tons (12-tons 18 cwt.) Total weight of armament, 4,500 tons. Weight of each broadside, if the guns do not revolve, is half of the above, or in the same case two 100-ton guns can be fired right ahead or right astern.

All the guns are mounted on Moncrieff's Hydro-pneumatic disappearing principle.

The decks throughout are plated with 2-inch iron properly hardened on the surface. Those over the gun-pits are supported on columns with hydraulic presses, and can be raised six feet or more for the guns to fire under them. They are also provided with apertures through which the guns can rise and fire when the decks are close down on the pits, on Colonel Moncrieff's plan. When the decks of the pits are raised only two or three inches, there is a continuous loop-hole, through which musketry can be fired in case of boarding.

The guns rest on a turntable of the full size of the gun-pit, in each pit. These turntables are composed of steel bars set on edge, and would stop shell fragments while not interfering with ventilation. Each turntable can revolve once in four minutes; thus forty pounds can be fired in ten minutes, with sufficient accuracy for all broadside work. The turntables are raised about two feet above the deck immediately below them. Small platforms project from the turntables, on which the sponging and loading is carried on, if muzzle-loaders are employed.

Disposition of Armour, Coal, and Engines.

Round the whole periphery of the hull there is iron equal to 30 inches' thickness, 30 inches deep; this may have a wood backing, if thought to be necessary, but at any rate will have 25 feet of coal behind that again. This armour-belt will have a weight of about 1,000 tons. The deck armour, 2 inches thick, will weigh 2,194 tons, and each additional inch of thickness about another thousand tons (or say 9 inches more draught of water). The larger gun-pit, with a diameter of 122 feet, will have a belt round it 4 feet deep and 30 inches thick. This will have a weight of about 800 tons. The smaller gun-pit, with a diameter of 100 feet, will have a similar armoured edge weighing about 680 tons. The conning tower, with an exterior diameter of 15 feet, will also be 30 inches thick, and will have a weight of about 40 tons. It would, no doubt, be desirable to have a belt below water for about 30 feet towards the keel, of at least two-inch iron, and it would not be difficult to provide this if it be made part of the structure and be not considered as external plating. The total weight

of such a belt would be 850 tons. The coal supply of 2,000 tons is carried all round the structure below water, but there is one point at the stern on the battery deck where the torpedo boats will interfere with it. The coal bunkers are divided as much as possible into water-tight spaces, and the same precaution is largely applied in the whole structure; but as the whole power of the engines can be made use of at any moment for the pumping out of water, and as a leak of 10 square feet takes 1,300 I.H.P. to meet the water flowing in, if the hole be 18 feet below the level—it follows that 16 such holes would fail to sink the vessel, so long as the engines could be worked, even if every hole were at the bottom, where nothing but a torpedo could make it. Or if a hole 10 feet square be caused by the torpedo, even two such could be met by resorting to higher pressure in the boilers such as would always be carried in action, since in boilers constructed on this system no increased risk is thereby incurred. As to the effects of ramming, it has been proved during the Confederate war in the United States, that on such a knuckle as this, the vessel that rams is even more likely to be damaged than the one that is rammed. (See a paper by Captain Hamilton, in the same volume of the U.S. Inst. Journal.) At any rate, the best possible provision is here made against serious damage from any such attack, while existing types have positively none but the interior watertight compartments which a previous unlucky shot may have rendered permeable, or the armour, badly disposed to meet such a blow.

Ventilation and Light.

Here there will probably be a great improvement over the *Devastation* type, since the gun-pits at all ordinary times may be like deck cabins, with glazed sashes all round the space, between what I may call their "lids," *i.e.* the moveable decks above and the rim armour of the gun-pit .In the captain's cabin and ward-room there may be shutters (iron-cased) which form skylights when not in place, and bullseyes in the deck must suffice for the smaller cabins. The ventilation will be aided by the engine-rooms opening under large spaces like the gun-pits, and in which up and down currents can be established by well-understood means.

The question of the behaviour at sea of such a structure as this will be always one of those on which various opinions will be held; but those who know anything of centre-board yachts will probably believe, with Mr. John Elder, that no vessel will combine stability with quietness in such a high degree. And this stability of vessel will also mean stability of gun platform, the two stabilities here coinciding instead of being obtained one at the expense of the other, as in former cases.

It will no doubt be said that this is not a ship. Anyone is at liberty to call it what he likes. All I can say is, that I believe it would answer its purposes, and that I am not alone in that belief may be seen by the many papers that have been written on similar forms by naval architects and others. I have advanced very little except a combination of improvements by various persons, which I have seen reason to think highly valuable; I have

ventured to depart from Mr. Elder's circular form, as Admiral Popoff departed from it in another direction—I hope with more chance of success, certainly for entirely different reasons. Even if this structure, costing about as much as the *Devastation*, were not possessed of gun power more than four times as great, it would certainly be able to turn more quickly and in less space and draught of water, and thus be a more formidable floating fort, leaving out of the question any difference of behaviour at sea. Either engine alone possesses a complete steering and propelling power, irrespective of any rudders, which might, however, easily be fitted if desired, like sliding keels.

As to torpedo boats, the fleet of wasps which must now be held to be a necessary adjunct to each unit of our naval power, there is here provision for nine such, all to be carried below deck. I almost think it is time to give up the carrying of any boats propelled by oarsmen in a man-of-war, if one pound of coal will give us one H.P. These torpedo launches can be raised on to the deck for launching, along projecting ways, like some of the life-saving appliances, and be got on board again in the same way, being afterwards lowered through iron-cased shutters in the deck to their ordinary place of stowage.

Armament.

No such battery has ever, I believe, been proposed even for a floating fort. Eight 100-ton guns, and eight 80-ton guns, all of which are available on any point of the compass, except the fore and aft line of the vessel, at any rate seem formidable enough for all probable requirements in coast defence ; and if, as I believe, such a vessel will answer as well at sea, then it would be hard to match her with any two deep-keeled ships carrying such guns and such armour, either on the broadside or in turrets, in point of cost, handiness, or light draught, besides the other advantages, almost all of which are due to the adaptation of Colonel Moncrieff's gunpits and hydro-pneumatic carriages, instead of turrets and recoiling carriages, however well these latter may be controlled by Captain Robert Scott's most ingenious and valuable appliances. I have taken the weight of each 100-ton gun with carriage and all equipments, munitions, &c., as 320 tons ; each 80-ton gun as 250 tons ; and this gives a total weight of armament of about 4,500 tons.

As each day's coal consumption on full steam will be about 200 tons I have not thought it necessary, in a sketch like the present, to go closely into all the weights, any excess in which an increased draught of water of a few inches at starting would amply provide for, without any great or prolonged inconvenience ; besides which, these calculations belong rather to naval architects than to seamen, as long as the two professions are not combined, as they ought certainly to be. As most persons will be disposed at first to consider this as a floating coast defence, it will be well to examine a few points in connection with the anchoring, grounding, &c.

As to the position of hawse-holes, I think it would be necessary to separate those for the bower anchors but little, there being no stem ; but those for the sheet anchors much more widely, the latter to be used exclusively in heavy weather, unless when moored ; in

that case the bower anchors could be shackled to the sheet chains, or *vice versâ*, in order to have the greatest buoyant power before or in front of the points of attachment. In grounding—which may sometimes occur with a coast defence ship, when she passes over shoal water where her adversaries cannot follow—the form of bottom here shown will facilitate getting off from sand or mud, as in such substances there will be only one point of contact, which the jet from the hydraulic will speedily excavate. There is no screw or rudder to destroy by bumping, and very little damage need be apprehended with a moderately soft bottom from lying aground. In a naval action the hydraulic propeller has great advantages; no wreckage can foul it, no unlucky shot can derange its after bearing, no rudder is necessary to it, and no great circle has to be described before a broadside or a ram can be brought into play. There is no order to be conveyed below, and perhaps misunderstood there, neither is there any reversing of engines in order to back the ship. All her movements are under the control of one master mind and hand, and if such a vessel ever comes into existence, it is my firm conviction that that mind, and that hand, will control the most formidable and the most powerful ironclad that has ever been floated on the ocean, and one which might successfully defy any two of the largest yet devised.

"Britannia" Ironclad.

Hydraulic engines, two sets (Perkins).

* Indicated horse-power	21,000
Coal supply	2,000 tons.
Speed	16 knots.
Number of days' full steaming	10
Distance in that time	3,840 knots.

The coal supply can be doubled if desired by giving an increased draught of water of eighteen inches at starting. The boilers are on Perkins' principle, and will give with a maximum of safety and durability one indicated horse-power with each pound of coal. The pressure on these boilers may be increased from 500 lb. on the inch to 1,000 lb. without any danger of bursting. If struck by shot no explosion would take place; no internal corrosion or scale ever occurs to diminish the strength, and render necessary a reduction of pressure. As the combustion in these boilers is slow and perfect, less heat will be felt in the engine-rooms, and less stoking will be required. Distilled water is used, and is re-condensed. The steering is entirely done by the engines on the late Mr. John Elder's plan of changing the direction of the issuing jets of water, fully described in the U. S. Institution Journal, vol. xii. (1868), No. 32, p. 520 *et seq.*, and to which able paper I refer those who may have any doubts on the means of steering or propulsion here advocated. As to the engines and boilers, the report of the Boiler Committee has been most favourable, and is confirmed by recent experience on a larger scale than ever before.

* The *Mary Powell*, a flat-bottomed paddle-wheel steamer, draught 6 feet, took 8 I.H.P. per square foot of mid section to drive her 20 miles per hour, 2,333 feet mid section at 8 I.H.P.=18,664 I.H.P. *Alexandra* takes 301 I.H.P. per square foot wetted area to drive her 15 knots; 60,000 square feet wetted area with 21,000 I.H.P. is equal to 350 I.H P. per square foot.

No doubt ought now to exist that one pound of coal per indicated horse-power per hour has become the standard from which no steamship engines ought to derogate. As with the compound engine, this economy has mainly been rendered possible by the use of more scientifically constructed boilers, in which much greater pressure can be safely carried. The engines have, however, a peculiar feature, in that they are run without any internal lubrication. The great heat of steam at 500 lb. on the inch pressure, would immediately carbonise any such substance as oil, and the metal or alloy which Mr. Perkins employs for his piston rings is therefore a *sine quâ non*, and seems to answer its purpose perfectly so far as it has yet been tried. The invention of this alloy would alone have been a most valuable improvement, but when we find that the expenditure of coal for the production of power has again been halved within one century, a debt of gratitude becomes due, first, to that Providence which has thus calmed our fears for the possible exhaustion of our fuel, and added to the means of that intercommunication which is the truest civiliser; next, to the inventor who, through much discouragement, and by many labours, has brought the invention with which he and his forefathers were charged to the stage of perfection at which we now find it.

The peculiar state in which the gaseous elements of water exist under pressures, and at temperatures such as are here spoken of, produces a most beneficial effect on the iron with which they come in contact. As Professor Barff has repeatedly shown, a peroxide is formed which is insensible to the action of even hot sulphuric acid, and we may expect also in a measure to the action of chlorine, the insidious enemy of all boilers, and which can with difficulty be eliminated from salt water by any ordinary distillation. The engines, and notably the condensers, should be proportioned to provide for a much higher number of revolutions than the ordinary working rate, as it is contemplated that in chase or other emergency increased pressure should be developed in the boilers. Steel should largely be used in substitution for iron in their construction, since some kinds of steel are only very good iron, and in the bearings aluminium bronze will be found a most valuable alloy. Every pump which is in connection with the boiler must be made with reference to the temperature of the gases—for it can scarcely be called steam—which exist in the boiler, or water-gas generator, as it may prove to be. Only last year in this Institution it was said that all improvements in high pressure boilers had proved failures, and the boilers of the *Montana*, *Dahcotah*, and others carrying 130 lbs. pressure were mentioned as instances. They were no doubt constructed under a most mistaken theory, and ought to have failed precisely as they did, but that does not invalidate the fact that for thirteen years boilers on the Perkins' system have been doing, and are still doing, satisfactory work with less than one pound of coal per indicated horse-power. The reason why is not difficult to understand. Mr. Perkins confines his whole boiler to three-inch tubes, which can easily bear the pressure he proposes to use. He then carries the expansion by heat of the water so far that the separation of its gaseous elements is complete, and part of the oxygen being absorbed by the iron to form a peroxide, the surface thereafter becomes insensible to any other oxygen, notably to that which we call rust, which is a lower combination of iron and oxygen. The proof of this is that when the boiler is opened after continued firing for the first time, the hydrogen which was in the heated state

abandoned by its oxygen in favour of the iron, with which alone it could combine at that temperature and pressure, can be lighted and burnt as a separate product, and that the iron inside the tubes is throughout coated with the peroxide, as above stated. But I believe it is fair to state that only Professor Barff and myself have, so far as I know, hitherto held this theory. It remains for others to explain the facts in some more perfect manner, if it can be done; meanwhile I have to apologise for this digression. At the same time, the possibility of having such advantages at sea as I have claimed for the most powerful ironclad, is largely dependent on the successful and economical use of very high pressures, and immunity from ordinary decay or fatal accident in the boilers; and more, this great advance, once thoroughly understood, will enable us to reduce the yearly expenditure, and double the efficiency, of all existing types of vessel.

TABLE I.—"*Dandolo*" *and* "*Duilio*" *Ironclads.*

Length between Perpendiculars	340 ft. 11 in.
Beam	64 ,, 9 ,,
Draught, forward	25 ,, 5 ,,
,, aft	26 ,, 5 ,,
Depth of Hold	21 ,, 11 ,,
Area of Midship Section	1,466·6 sq. ft.
Weight of Unarmoured Hull	3,395·5 tons.
Displacement	10,401 ,,
Total Weight of Armour	2,559 ,,
Wood Backing	239 ,,
Number of Compartments	102
Diameter of Interior of Turrets	25 ft. 9 in.
Thickness of Armour at Water-line	21·65 in.
Thickness of Wood-backing at ditto	23·62 ,,
Thickness of Armour in Turrets	17·71 ,,
Thickness of Wood-backing in ditto	11·81 ,,
Weight of Guns, their Machinery and Ammunition	984·2 tons.
No. of Guns	4
Weight of each Gun	98·5 tons
Weight of Broadside Metal	3·87 ,,
Bow fire	2·9 ,,
Stern fire	1·93 ,,
Length of Guns	32 ft. 10½ in.
Breech Diameter	77 in.
Muzzle Diameter	29 ,,

[TABLE II.

TABLE II.—*Engines, &c.*

	"Dandolo."	"Duilio."
Builder	Maudslay	Penn.
I.H.P.	7,900	7,500
Engines	Compound	Ordinary.
Revolutions	80	65
Propellers	2	2
Diameter	16 ft.	17·3 ft.
Pitch	19 ,, ...	19·6 ,,
Speed	14 knots	14 knots
Grate Surface	811·6 sq. ft.	899·6 sq. ft.
Heating Surface	22,991	23,775
Pressure	60 lbs.... ...	30 lbs.
Coals in Bunkers	1,279 tons	1,279 tons.
Consumption per day	211 ,, ...	321 ,,
Full Steaming	6 days	3 days 20 hours.

TABLE III.—"*Italia*" *Ironclad.*

Length	400 ft.
Beam	73 ,, 10 in.
Draught, forward	25 ,, 4 ,,
,, aft	30 ,, 4 ,,
Height of Upper Deck above Keel	49 ,, 3 ,,
Area of Midship Section	1,848 sq. ft.
Weight of Unarmoured Hull	5,000 tons.
Displacement	13,480 ,,
Double Bottom—length	254 ft. 3 in.
,, width	59 ,,
,, depth	3 ,, 3 ,,
Two Longitudinal Bulkheads, Water-tight Compartments Forty of these above the double bottom, three in the rear and ten forward of it.	53 in number.
Four water-tight decks: lowest plated with iron 3 in. thick, 8 ft. 2 in. below water-line; second deck 5 ft. above it; battery deck 14 ft. 9 in.; and upper deck 19 ft. 6¼ in. above.	
Twin Screws.	
Engines, Ind. H.P.	18,000
Calculated speed	16 knots.

EXTRACT FROM A LETTER ON STEAM GUN-BOATS OF SHALLOW DRAUGHT AND HIGH SPEED.

THE following letter was addressed to a scientific friend, with a view to its being read at the late meeting at Liverpool of the British Association, where I expected that I should see those gentlemen whose adverse opinions, on a former occasion, I had endeavoured to meet in the hope of clearing from difficulty, and bringing before the Government and the Public, a most important subject. My friend,

however, unfortunately could not attend, and the Chairman of the Mechanical Section, before whom it would have been brought, had previously received too many subjects, and had treated this subject with too much ridicule for me to attempt to press it upon him :—

15, *Park Street, Westminster.*

My Dear Reade,—Now that the advantage—nay, even the necessity—of having war-vessels of light draught, but of comparatively heavy burden, driven at considerable velocity, admits of no doubt; and, considering the treatment of my Paper of April last, I may surely be excused for attempting in another place to show publicly that the mode of propelling vessels by the reaction of a jet of water possesses considerable advantages, and has been unjustly condemned.

The paddle-wheel, made as large as it should be for the necessary speed, offers too fair a mark to the enemy, and the screw cannot be applied to its full advantage in vessels, of shallow draught; but to the propulsion by the jet I see neither of these objections apply.

But, we are told, the method of propelling vessels by pumping has been proved by many eminent engineers, by very many and very costly experiments, to be a failure; and, after the appearance of my theory, which I believe to be the true theory, and which I have with great pains tried to explain to those persons who, one would think, were most capable of understanding it; still, as before, some say, "There is nothing in it—it is all a mistake—it is ridiculous;" others say, "There may be something in it, but there is even in theory a loss of one-half the power; "Mr. Gravatt's theory is all blunder;" and all make inconceivable mistakes in what they call the result of experiments and in the theory.

Without further preface, let—

$v_{,}$ = velocity of the boat, in feet per second, through still water.

$v_{,,}$ = velocity of the tail-water, or jet, with regard to the boat.

$a_{,}$ = area in square feet of the head water.

$a_{,,}$ = area in square feet of the tail water.

p = pressure in lbs. to keep the boat at the velocity v.

P = lbs. × feet per second, engine power (which, divided by 550, gives horse power).

E = $p\,v_{,}$ lbs. × feet per second, which, divided by 550, gives effective horse power;

and note, that as a cubic foot of sea-water weighs 64·18 lbs., and 2 g is so nearly the same, in all cases they are taken as equal, both being written 64.

The head due to $v_{,} = \frac{v_{,}^2}{64}$, and the constant pressure to bring the head water to a state of rest, with regard to the boat, is,

$$\frac{2\,v_{,}^2}{64} \times 64\,a_{,} = 2\,v_{,}^2\,a_{,}\text{ lbs.}$$

The head due to $v_{,,} = \frac{v_{,,}^2}{64}$, and the reacting pressure of the tail, is,—

$$2\,v_{,,}^2\,a_{,,}\text{ lbs.}$$

$$\therefore p = 2\,v_{,,}^2\,a_{,,} - 2\,v_{,}^2\,a_{,}\text{ lbs.}$$

The pumping power is $64\,v_{,,}\,a_{,,}$ lbs. raised $\frac{v_{,,}^2}{64} - \frac{v_{,}^2}{64}$ feet per second $= v_{,,}\,a_{,,}\,(v_{,,}^2 - v_{,}^2) = \text{P}$.

Now put $v_{,,} = n\,v_{,}$, then, as the same quantity of water goes out as comes in,—

$$v_{,,}\,a_{,,} = v_{,}\,a_{,};\ \therefore a_{,,} = \frac{a_{,}}{n};$$

$$\therefore p = 2\,(n-1)\,v_{,}^2\,a_{,},\text{ and E} = 2\,(n-1)\,v_{,}^3\,a_{,},$$

$$\text{P} = (n^3 - 1)\,v_{,}^3\,a_{,}\text{ and }\frac{\text{E}}{\text{P}} = \frac{2\,(n-1)}{n^2 - 1}.$$

Now when this expression is a maximum, it is of the form $\frac{0}{0} = 1$, and when $n = 1$, $E = 0$, $P = 0$, $a_{,,} = a_{,}$.

But

$$\frac{2(n-1)}{n^2-1} = \frac{2(n-1)}{(n+1)(n-1)} = \frac{2}{n+1} = \frac{E}{P}.$$

Now if

$$n = 10, \qquad E = 18\, v_{,}^3 a_{,,} \qquad P = 99\, v_{,}^3 a, \quad \text{or } \frac{E}{P} = \frac{2}{11};$$

but when

$$n = 1{\cdot}5, \qquad E = v_{,}^3 a_{,}, \qquad P = 1{\cdot}25\, v_{,}^3 a_{,} \frac{E}{P} = \frac{4}{5}.$$

Now, without going further, take as an example the *Medea* paddle-wheel steam frigate, of 843 tons burden, and 14·5 feet draught, where it has been said,—

$$p = 4500 \text{ lbs. } v_{,} = 16,$$

and the horse-power 220, but which I believe is nearer 330.

Let $\quad n = 1{\cdot}5$, then $p = 16^2 a_{,} = 4500$, or $a_{,} = 17{\cdot}6$ and $a_{,,} = 11{\cdot}7$,

$$v_{,} = 16_{,}; \; v_{,,} = 24.$$

$$\text{The horse power} = \frac{1{\cdot}25}{550} \times 17{\cdot}6 \times 16^3 = 164 \text{ nearly.}$$

This result is, of course, exclusive of friction; but here is a large margin, although n is not taken so small as it might be; and when we consider the draught of water for this mode of propulsion need not be more than 4 feet, the advantage for vessels of light draught is evident.

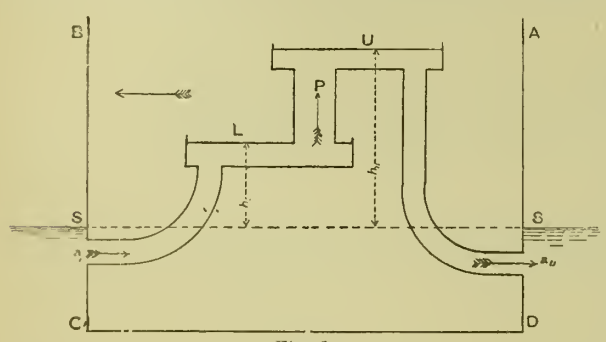


Fig. 1.

Now to persons really conversant with hydrodynamics, the foregoing is all and more than I should offer; but as a familiar way of conceiving the matter, we may consider figure 1.

In the diagram Figure 1, let A B C D represent a vessel moving through the water in the direction A B, with a velocity v'. Let S S represent the surface of the water; C D being the bottom of the vessel; L, the level of the water in a tank, called the lower tank; U, the level of the water in a tank, called the upper tank; P, a pump lifting the water from the lower tank to the higher.

The arrows show the direction the water takes.

The water is made to enter the vessel at $a_{,}$; is brought to a state of rest with regard to the vessel, and raised to a height $h_{,,}$.

Whatever causes this state of things, may properly be called the Cause.

The water descends from the height $h_{,,}$, and flows out at the stern at $a_{,}$; and whatever be the effects produced, the sum may properly be called the Effect.

Now the vessel moving with the velocity $v_{,}$, the pressure at $a_{,}$ is measured by $2\, h_{,} a_{,} = \frac{v_{,}^2 a_{,}}{32}$, and as the quantity of water flowing out is equal to that coming in, we must have $a_{,} v_{,} = a_{,,} v_{,,}$ or $a_{,} = \frac{v_{,}}{v} a_{,,}$, and the pressure $= \frac{v_{,} v_{,,}}{32} a_{,,}$.

The relative velocity of the water being $v_{,,}$ the cause of the water flowing into the lower tank L, and remaining there relatively at rest, will be properly measured by $\frac{v_{,}^2 v_{,,}}{32} a_{,}$.

Again, the lift from the lower to the upper tank U is $h_{,,} - h_{,} = \frac{v_{,,}^2 - v_{,}^2}{64}$ and the cause of the water rising, this stage is properly measured by $\frac{v_{,,}^2 - v_{,}^2}{64} v_{,,} a_{,,}$.

The sum of these causes $\frac{v_{,}^2 v_{,,}}{32} a_{,,} + \frac{v_{,,}^2 - v_{,}^2}{64} v_{,,} a_{,,} = (v_{,,}^3 + v_{,,} v_{,}) \frac{a_{,,}}{64}$ is, therefore, the proper measure of "The Cause."

Now the water flowing out of the upper tank will have a velocity due to $h_{,,} = \frac{v_{,,}^2}{64}$, and the counter-pressure will be $\frac{v_{,,}^2}{32} a_{,,}$; and as the vessel moves with a velocity $v_{,,}$ this part of the effect will be measured by $\frac{v_{,,}^2}{32} a_{,,} v_{,}$.

But, besides this, the velocity in space of the effluent water will be $v_{,,} - v_{,}$, and this effect will be measured by $\frac{(v_{,,} - v_{,})^2}{64} v_{,,} a_{,,}$.

The sum of the effects, or "The Effect," is, therefore, properly measured by

$$\frac{v_{,,}^2}{32} a_{,,} v_{,} + \frac{(v_{,,} - v_{,})^2}{64} v_{,,} a_{,,} = (v_{,,}^3 + v_{,,} v_{,}) \frac{a_{,,}}{64}.$$

The expressions for the cause and the effect being *identical*, obtain whatever may be the absolute or relative values of $v_{,}$ and $v_{,,}$, and so far show the parts have been rightly determined.*

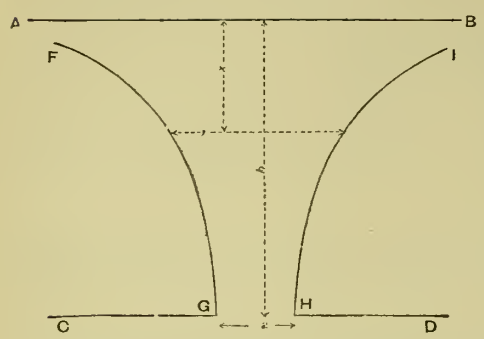
This mode of dividing the cause and effect into parts and showing the identity of the expressions for the sums (although not necessary any more than the diagram), is, I conceive, instructive, and enables us to see pretty clearly what is going on, and how to apply the formula practically.

The matter is of so much consequence that I must, however unwillingly, make some remarks on the very different results obtained both theoretically and practically by other engineers, and to notice a paper of some length (in which I am directly censured and supposed to be confuted) presented by Mr. Brunel to the Institution of Civil Engineers, in answer to the foregoing, which, with two other ways of considering the matter, I laid before the Council, and exhibited at an Ordinary Meeting of the Society in April last, but which the perhaps natural impatience of a large assembly, not all scientific, at any mathematical subject, the noise of a party, and preconceived ideas of my being in error, prevented their attending to.

In the first place, the fact originally discovered by Newton from theory, that the counter-pressure of an effluent stream of water is double the pressure of the column of fluid due to the velocity, was either wholly unknown, directly disputed, or even after my demonstrations and practical proof, considered false, or at least an unaccountable mystery.

I shall, therefore, in order to clear the ground, give the demonstrations I exhibited; and, first, that borrowed from Newton, although I shall use his fluxional method instead of his method of prime and ultimate ratios, used by him in his demonstration.

* "You with whom I have talked over the logic of the higher branches of mathematics and physics, know the stress I in certain cases put upon the difference between identity and mere equality, and have heard me more than once remark, that if you say to a man, 'The effect is only half of the cause,' he will immediately assent, though he would rather stare if you told him the *cause* was only half the *effect*."



Let A B be the surface of the water in a vessel of unlimited extent; C D the bottom of the vessel. Let h be the depth of the water, and a the diameter of a circular hole in the bottom through which the water issues; let also s = contents of the cataract F G H I.

Now, considering the figure, the velocity of the water at a will be $8\sqrt{h}$, and the velocity at y will be $8\sqrt{x}$; and as the quantity of water passing any point must be equal, we must have $\delta\ s = y^2\,\delta\,x = \frac{h^{\frac{1}{2}}}{x^{\frac{1}{2}}}a^2\ \delta\,x$, which when $x = h$, gives $s = 2\,h\,a^2$.

Now the contents of a column whose base is a, and height h, is only $h\,a^2$, and the contents of the cataract being twice this, and all the water in the cataract freely (except from friction) obeying the force of gravity, the bottom of the vessel will be relieved from the weight of a column of water $2\,h\,a^2$, as published by Newton in the year 1686.

My other demonstration was as follows :—

Suppose a mass, m, be acted upon for one second by a force fm, then the velocity acquired at the *end* of that time will be $v = 32\,f$ feet per second, and the space the force acts through will be $16\,f$.

Now, if another similar mass be similarly acted upon, the same result will follow.

Suppose each mass m to be a cylinder, a circular feet base and $16\,f$ feet long, then the two masses might form a continuous cylinder $32\,f$ feet long, moving uniformly at the rate of $32\,f$ feet per second.

Each mass would react on a piston whose area is a, with a force $fm = 16f^2\,a$, and the sum of the mechanic power employed would be $32f^2\,a \times 16f = 16 \times 32\,af^3$. Now

$$v = 32f,\ \therefore f = \frac{v}{32},$$

and the sum of the pressures will be

$$32\,\frac{v^2}{32^2}\,a = \frac{v^2}{32}\,a;$$

that is, *twice* $\frac{v^2}{64}\,a$: and this demonstration being wholly independent of any laws of fluids, I cannot conceive how anybody, after hearing it, can be puzzled, or think that any mysterious laws of fluidity can be involved, or that what is called the double pressure "is, as it were, an accident."

Mr. Brunel, who has also, it is said, *proved* by experiment the failure of the principle, puts v = the velocity of the boat through the water; $v_{,}$ that of the effluent water with regard to the boat; and a = area of jet.

The power expended in raising the water,

$$a\,\frac{v_{,}^3}{64};$$

the power employed in taking up and carrying the water to be ejected,

$$\frac{v^2\,v_{,}}{32}\,a;$$

reacting pressure of the jet,

$$a\,\frac{v^2}{32};$$

and the power elicited,

$$\frac{v_{,}^2}{32}\,a\,v.$$

Now, applying my test, the velocity of the effluent water in space is

$$v_{,} - v,$$

and the effect will be

$$\frac{(v_{,} - v)^2}{64} v_{,} a;$$

the sum of the causes

$$\frac{v_{,}^3}{64} a + \frac{v^2 v_{,}}{32} a = (v_{,}^3 + 2 v^2 v_{,}) \frac{a}{64};$$

the sum of the effects

$$\frac{v_{,}^2}{32} a v + \frac{(v_{,} - v)^2}{64} v_{,} a = (v_{,}^3 + v_{,} v^2) \frac{a}{64};$$

which expressions are not identical; and, therefore, "the cause" may be made to bear any ratio whatever to "the effect," which is absurd.

The only case in which the cause and effect are equal is when $2 v^2 v_{,} = v^2 v_{,}$, or when $2 = 1$, which is absurd.

Mr. Brunel takes as an example the case of a boat moving at a velocity of 25 feet per second (or 15 knots per hour), and the velocity of the tail water at 75 feet per second, which, as he says, corresponds to a head of 88 feet.

Let us add, for the sake of completing the example, that the pressure to keep the boat in motion at that velocity is 10,000 lbs.

The actual horse-power required is	455
And by Mr. Brunel's theory,—	
The actual horse-power employed is	1024
The horse-power wasted	569

The head of water required being 88 feet.

Now, by my theory, the useful horse-power is	455
The actual horse-power required is	512
The horse-power wasted	57

Only one-tenth of that wasted according to Mr. Brunel.

And the head of water required is 15 feet, instead of 88 feet, as calculated by Mr. Brunel.*

According to Mr. Bidder, and those who will insist that the reacting pressure is merely the weight due to the height of the column, we shall have (using my notation and applying my test),

$$\frac{v_{,}^2 v_{,,}}{64} a_{,,} + \frac{v_{,,}^2 - v_{,}^2}{64} v_{,,} a_{,,} = \frac{v_{,,}^3 a_{,,}}{64},$$

the expression for the cause, and

$$\frac{v_{,,}^2 v_{,} a_{,,}}{64} + \frac{(v_{,,} - v_{,})^2}{64} v_{,,} a_{,,} = \frac{v_{,,}^3 a_{,,}}{64} + (v_{,}^2 - v_{,,} v_{,}) \frac{v_{,,} a_{,,}}{64},$$

the expression for the effect.

These expressions not being identical, the ratio of the cause to the effect may be made to vary, which is absurd.

* Mr. Brunel says Smith's screw-propeller (meaning, I suppose, the screw-propeller) had been as clearly proved bad by experiment and theory as the jet.

That badly-directed (however costly) experiments, both with the screw and with the jet, have been attended with bad results, I have no doubt; but where by theory was it that the screw-propeller (not Smith's screw-propeller) was proved bad? Tredgold, in 1827, publicly advocated the use of "the screw" from theory, and, moreover, has these remarkable words: "I can easily conceive that the trial of an experiment may be the means of condemning a very useful principle merely through inattention to the proportions and mode of action."

That the principle is good, and that the attempts at experimental proofs and theoretical proofs of its insufficiency are both bad, I think I have sufficiently shown. That the matter at this crisis can be allowed to rest here is impossible; a few *properly-directed trials* would show the Government and the country, that "the stupid old-as-the-hills attempt at pumping a boat forward" might BE OF ENORMOUS SERVICE.

Yours faithfully,

To the REV. J. B. READE, *Stone Vicarage.* W. GRAVATT.

DISCUSSION.

Mr. JOHN R. RAVENHILL: My Lord, there are one or two questions I should like to put to Admiral Selwyn, if he will kindly answer them at the end of the discussion. I am not going to allude to the ship, but the remarks which I am going to make will be confined to his coal consumption, and the boilers which he proposes to adopt. He states: "No doubt ought now to exist that 1 lb. of coal per indicated horse-power per hour has become the standard from which no steamship engine ought to derogate," and he laid emphasis on the words "to derogate." If he will oblige us this morning by telling us of any one vessel running for a time over a series of voyages at anything like that consumption, I am sure he will confer a favour on all the members of this Institution. With regard to his remarks that were made last year on high-pressure boilers having proved valueless, I think I must take the opportunity of correcting him in one important particular, and that is this, that the boilers which were spoken of as having been valueless were water-tube boilers. He speaks as if all boilers carrying 130 lbs. might have been so.

Admiral SELWYN: I refer to the *Montana* and the *Dahcotah*, and they were distinctly water-tube boilers.

Mr. RAVENHILL: The boilers of the *Montana, Dahcotah,* and others carrying 130 lbs. pressure, were mentioned as instances. The paper is not quite clear as to whether you only meant water-tube boilers, or included others as well. A few lines further on it is stated "that for thirteen years boilers on the Perkins' system have been doing, and are still doing, satisfactory work with less than 1 lb. of coal per indicated horse power." I must confess that startled me when I read it, but, to a certain extent, in reading this paper Admiral Selwyn has qualified it by introducing *vivâ voce* the words, "on land." But in bringing this under our notice I am sure Admiral Selwyn must be well aware that the only boiler on Perkins' system that has been to sea came, to a certain extent, to grief, and has formed the subject of an action in the law courts. I myself was professionally consulted about it, and the tubes are actually in existence that were silted up. Whilst I state that, I think it also right to mention that lately a boiler has been passed by the Board of Trade officials at Newcastle-on-Tyne, and has received a three months' certificate, the time of which is now nearly expired, for a passenger steamship running on the Tyne, but I certainly know of no case (and I believe I am pretty well posted in these matters) where Perkins' boilers have run at sea. I will not say they may not have come round the coast, but I mean working at sea, and taking a voyage across the Bay of Biscay. Any information Admiral Selwyn could give us on that point would also be very valuable, because the result he starts with here—going back to his 1 lb. of coal—is, that one H.P. is to be produced, and can be produced, by Perkins' boiler; but it is certainly very important that we should have the fullest accounts as to the true practical working efficiency of that particular class of boiler. I am merely asking for information, but I felt it right to mention the facts which I have stated.

Mr. Nathaniel Barnaby: It may be convenient, my Lord, if I rise early in the discussion. I may naturally be supposed to be rather a severe critic of the proposal which has been brought before us this morning, because, to some extent, it comes upon my ground; and before making some general remarks upon the design, I should like, following up what Mr. Ravenhill has said, to mention a fact with regard to the peroxide which Professor Barff relies upon for the protection of iron surfaces. It has been known to those who have used steel considerably, that under some circumstances steel, when employed in salt water, takes very severe pitting, and objections have been urged against the use of steel for ships on the ground that, somehow or other, in salt water steel would not last so long as iron, that it pitted, whereas iron would not, and we found a certain confirmation of that in some experiments which we made, and we could not at first see what was the reason of it. It seemed reasonable to suppose that steel would take corrosion uniformly over its surface, and you would expect that iron would have the more severe pitting, and not steel; but it turned out that there was a certain black oxide adherent to the surface of the steel, and much more closely adherent to the surface of steel than to the surface of iron. It is produced under the rolls. It is the very same thing as that which Professor Barff relies on for the protection of his iron, and we discovered this, that there was a strong galvanic action between that black oxide and the steel itself, and if you get a small portion of that black oxide removed from off the plate, and put the plate into salt water, you might as well have had the plate coated with copper as to have allowed the black oxide to have remained on it. Now I say that is the same as Professor Barff does, and I say that because we actually tried some specimens as prepared by Professor Barff to see what happened to them when they were put in salt water. We saw that the process had a certain good effect in air, but as soon as we put it in salt water we discovered that rust was rapidly formed, and when we wiped the rust off we found there was deep pitting taking place immediately, and, as a matter of fact, that the peroxide that had been formed on the surface of the material for the purpose of protecting it was an enemy that was eating into the heart of the material directly one small exposed place presented itself on the surface. I am sorry I did not bring here this morning a plate which I have in the office—a piece of steel which was produced under the rolls, with a good black oxide on, which has been immersed in salt water for about seven or eight months, and I am quite sure that shipbuilders here would have been astonished if they had seen the condition of the surface of that piece of steel. It was pitted in a frightful manner. Apparently, when it was put into salt water the surface was quite perfect, but there were minute openings in the black oxide, and as soon as ever the action commenced it spread and went deeply into the material. I need hardly therefore say that those who propose to use salt water on the surfaces of steel must beware how they allow anything like this black oxide to remain on the surface of the material. And now a word or two about the ship itself. What I should like to say is, that if any of the eminent shipbuilders here could get some foreign Government to build the ship I think we should all be very pleased, but if I were asked by the English Government what I thought of it I should say, Well, gentlemen, if you believe what has been said you cannot do better than build the ship yourselves, but do not make me responsible for it, because you certainly will not get the things done which are said, nor anything like them, and, in my opinion, you will be greatly disappointed with the result. However, I will just give you one or two facts concerning it. First with regard to the cost: you must fairly face that. You are told she will not cost more than the *Devastation*. Now the *Devastation*, I find, cost for hull, machinery of all kinds, rigging, and stores, £360,000. Now the engines of this ship alone, 21,000 horse-power, would cost a quarter of a million of money. The armour alone would cost well on to the same amount, and if you put the engines and the armour together you will very greatly exceed the cost of the *Devastation*, without leaving anything for building the ship herself. That would not so much matter, perhaps, if the ship were built in such a light manner as Admiral Selwyn

supposes, but I think he has greatly underrated the weight of the ship that he puts before us: I mean the weight of the hull. If you compare the figures which he gives for the weight of the hull of his own ship, and the proportion they bear to the displacement, with those which he gives on the last page for the weight of hull compared with displacement of the Italian ships, you will see he has taken credit for some most wonderful performance in the way of building a light hull. You will see on page 217 that in the *Dandolo* and *Duilio* the weight of unarmoured hull is given at one-third of the entire displacement. If you look on page 218 you will see that the weight of the unarmoured hull of the *Italia* is considerably more than one-third of the displacement. But if you look at the weights taken by Admiral Selwyn for his own hull you will find it is only 2,600 tons out of a displacement of 14,767 tons. I do not know how that is to be managed. Now when one challenges, to begin with, the estimated cost of the ship to the extent which I have done, and then challenges, in the next place, the assumed weights to the extent to which they may be challenged, as one may see by looking at these figures here, to say nothing of any others, and when one looks at the novelties in the design, I think it would be a bold Government in England to undertake it, and I think that what they would probably say with regard to it is, that it is declined with thanks.

Captain Dawson: My Lord, I should like Admiral Selwyn to explain the dotted lines in the profile, and the dotted circles on the plan, because I do not quite understand what they mean.

Mr. J. D'Aguilar Samuda, M.P.: My Lord, it is always a very ungracious task to have to find fault, or to say what appears to be unkind things, of any proposal put before us with the amount of care that this has evidently had at the hands of the author; but I would, in the first instance, point out that I think we ought not to have placed in the same proposal two or three propositions so totally at variance with that of the existing practice. If those propositions happen to be right the existing practice ought to have the advantage of them in any comparison with the new proposal; or, in other words, to put it in a more practical form, that a proposition for a ship, if it has any good in it, ought to be dealt with with engines such as are universally known and accepted. A proposition for engines, perfectly new, ought to be dealt with in a ship of a class which is well known; but when you deal with engines, and make a proposal to adopt engines with conditions which have never hitherto been attained, in a ship which is so unlike anything which has previously been done as to call for the observation from Admiral Selwyn himself, "I do not know whether you would call it a ship or no," then I think we have too many novelties pressed upon us at once, and the duty is, unnecessarily, pressed upon those who attempt to discuss or criticise it, to separate out the advantages of all these different proposals, when they ought to be at liberty only to deal with one. Mr. Ravenhill has made great reference to the engine question, but I would look at the engine question from a different point of view from Mr. Ravenhill, to this extent. I believe there are many valuable advantages to be obtained by Mr. Perkins' plan, and I by no means doubt but that, some time or other, that plan will be considerably more used than now; but if you have to deal with a ship with ordinary engines, and put aside Mr. Perkins' engines which I think you are bound to do, for the reason Mr. Ravenhill has given that no engine exists working in a vessel at sea anything at all approaching to that which is here described; and then, if you accept the best engines which we have working at the present moment at sea, you immediately get rid of the possibility of being able to carry ten days' fuel, and you reduce it down, even with the same-horse power here indicated, to something under three days. Then, again, when you come to deal with the ship itself, the ship is compared with the *Devastation* favourably, but it is quite clear that the writer of this paper contemplated that speed should be an important element in his invention. But speed is impossible with that ship—absolutely impossible. I have shown before in this room, when the Russians brought forward their plan of the circular ironclad (and this has all the disadvantages, in point of form, which those circular ironclads have), that it was absolutely impossible,

put what power you please into the ship, to drive it at fourteen knots, and here is a proposal to drive this one at sixteen. They made very strong reference to Mr. Froude, who had been making a number of experiments for them, to confirm their statement that they could drive their ships at this speed, and Mr. Froude remained silent. But it may be within the recollection of this meeting that on the subsequent occasion, when the matter was again discussed, and Mr. Froude was pressed to give the results of his experiments, he admitted distinctly that if the ship were driven at ten knots it would go down, that the water would go over the ship. So it would be with this ship. The whole of the attention, the skill which has been devoted for the last half-century to improving forms of ships, is entirely ignored by attempting to bring forward proposals of this sort, and putting them before naval architects as vessels capable of going sixteen knots. Here are we working away, and doing everything we can do to change the forms of vessels advantageously. I need not point out in an Institution like this what would be the result of these changes; but this we know, that when we started with bows somewhat similar to what this diagram shows—I recollect it perfectly—when we started with the bluff bows that we had in old sailing ships, and applied them to steamers in the early days to go from London to Ramsgate, the circumstance of merely drawing out the bow and making it sufficiently fine, to halve the angle of incidence, shewed that with the same power you exactly double the effect; that is to say, where you could only drive a boat eight knots in one case, you were able to drive it ten knots by simply making the alteration, with the same, power of lengthening the bow from the old circular bow, so as to be able to make the angle of incidence one-half. There is not time to drive the water round these bows, consequently it must mount over. You are asked to consider a case which has many novelties which cannot refer in the least degree to practice, except, allow me to say, most unfavourably, because those circular ironclads which were built, and actually put to work, were shown to have all the disadvantages in speed that I predicted they would have, and therefore confirmed all those views which not only my suggestion, but Mr Froude's experiments also confirmed; thus, it does not appear to me wise, to discuss a paper of this sort without restricting the consideration to one novelty at a time. I find, throughout the paper, that there is a continual reference made to Mr. Elder. I do not know that it is a very important matter, but we are left in doubt whether this was Mr. Elder's proposal or not. I do not think it was. I think, if I recollect right (and let me say that, for the advantage of this proposal), that Mr. Elder claimed to have exactly the same proposal as the circular ship—the absolute circular—and, in that respect, he was even worse still; but, in any case, the form which is here given to the bow is of such a nature that it does appear to me to be idle that we should discuss, with any probability of being able to come to a satisfactory conclusion, that anything like sixteen knots an hour could be predicted with any amount of power that could be forced into the ship.

Mr. J. Scott Russell, F.R.S.: When Admiral Selwyn replies, I should like him to state in his answer very clearly why he adopted two turrets instead of one—or two batteries, I will call them, instead of one; and, secondly, what his reason is for departing from the simple circular form of hull into this oval one; because two turrets have the horrible disadvantage of each sticking in the way of the other. It is a great inconvenience, and it seems to me that a good many of the advantages of what we call the pure Elder construction, or what we call Admiral Popoff's construction, are lost when you deviate from the pure circle. I venture to submit this because it has been stated, on more occasions than one, that I had entirely approved from the beginning of Elder's form, and entirely approved from the beginning of Popoff's form. There is no doubt that I entirely approved of Popoff's form and entirely approved of Elder's form, but I have never approved of them as ships. I never approved of them as vessels to cross the ocean; I never approved of them as fast steamers; I never approved of them as economical steamers; and I never approved of them as able to make good their way in the conditions in which I hope all

English ships of war will always make good their way, in a heavy storm with a heavy sea on the wide Atlantic, or, what I dislike still more, having endured more of it, the Bay of Biscay. Now three days and three nights in the Bay of Biscay, either upon Elder's boat or upon Admiral Popoff's vessel, or I fear upon Admiral Selwyn's vessel, would see her—I was going to say, nowhere, but I will say ashore —would see that ship on the shore. I have been, for example, three days and three nights steaming full power ahead against a head wind and a heavy sea in the Bay of Biscay, and at the end of those three days and three nights we were fifty miles astern of the place we had been at when we started. Now that is the sort of thing that a ship has to encounter, and I think you will allow me to go back to the disagreeable thing I said before, that you may call it anything you like if you will be kind enough not to call it a ship. Then I will only mention one case more. There was a grand series of vessels of which we have all been proud, built by Mr. Samuda and several other gentlemen, at a time when such ships had already been pronounced to be impossible. Those ships have remained our pride ever since; and what is the quality of those ships?—I mean the ships between Holyhead and Dublin. What is the peculiarity of those ships? The peculiarity of those ships is that they are nearly as good in a heavy storm as in a calm sea. I entreated that Company to allow me the pleasure of an experiment, and to get up a good storm for me. They telegraphed to me that it had been blowing cats and dogs for three days and three nights, and they had never seen the like of it, and they thought I had better come down at once. I went down at once. I went over in one of those wonderful ships. I was tied, with the captain and the steersman, because nobody else was able to remain on deck, to the bulwark, and we stood up on that platform. I saw the vessel go through seas in which 120 feet of the length of the vessel was under water at the time: we went straight through, and we accomplished this work within once and a half of the ordinary time of a smooth water voyage. Now no such performance as that can ever come out of a vessel of this class; and I am afraid, to build a seagoing ship you must go back to what we technical fellows call a ship-shape, and not a tub-shape.

Admiral Selwyn: My Lord, first I have to answer Mr. Ravenhill's questions, and I am afraid that the persons who properly should have answered them, Mr. Perkins and his friends, are not here. No doubt they would be able to give a sufficient answer, and that answer would be that the *Filga* has been built ten years. The first thing done with her was to send her out to Malta, and she has been running as a tug ever since, and nobody ever complained of her, or ever wanted new boilers put to her, which is much more.

Mr. Ravenhill: I cannot allow that remark to pass quite unchallenged: I hope that I am not out of order. The *Filga* ran for many years in the Thames here, and it was on being sent from the Thames and going across the Bay of Biscay she came to grief, and had to put into Vigo, where she lay for many months for repairs before she was able to continue her voyage to the Mediterranean, and that voyage has been the subject of an action in our law courts in London, and was reported in the *Times* in December last.

Admiral Selwyn: I am not so capable a defender as Mr. Perkins would have been, but I may tell Mr. Ravenhill that the reason why the *Filga* on that occasion came to grief was simply because the engineer chose to consider that, instead of doing as he was told and using condensed water for feed, he should get it out of the sea, and the instant he did that, which was a direct violation of the whole principle on which the thing was constructed, he came to grief, as anybody would who attempted to use in these boilers anything else than distilled fresh water. The principle is entirely one of using the same water over and over again, without applying to the sea for any feed at all. Those who do not know that can conceive the engineer fancying the ship was in danger.

Mr. Ravenhill: The fresh water became exhausted.

Admiral SELWYN: Possibly. If any engineer behaved as he would do with ordinary boilers, by opening the taps and wasting the steam, he would inevitably waste the water supply, and would be forced to resort to a salt water feed. But that is not the right way to do it. I instanced the *Montana* and *Dahcotah* specially, because they were water tube or *tubulous* boilers designed to carry a higher pressure than was ever carried before. I do not say at all that high pressures cannot be carried in ordinary well-fitted boilers, but no such pressure as we speak of here has ever been thought of at all, and no such results have been attained as were here attained. I think Mr. Ravenhill will allow that when a steam boiler performs its work satisfactorily for thirteen years on land, at least it is a great encouragement to try what can be done with the same boiler at sea; and when added to that we have such small experiments as could be looked for made by the inventors themselves, the proposal, although one involving novelty, would be a very great benefit if it could possibly be adopted, and therefore deserves a fair trial by Government. I think they can fairly rely on the Report of the Boiler Committee specially appointed to investigate that question, which says it ought to be tried on a larger scale. However, as that was not done—for what reason I cannot say—a still further advance has been made by private enterprise, precisely as in the case of most of our great advances in the marine of England. The screw would never have been adopted at all against the persistent resistance of those who refused to see anything in it, and believed in paddles, had it not been for the persistent efforts of private enterprise. I thank God that there is such a thing as persistent private enterprise in this country. Mr. Barnaby says that the peroxide applied outside the ship—such an imperfect peroxide as the rolls make, not deposited by any such action as this—always results in a galvanic action. I have studied that question, and I think I should ask him what it is that causes pitting? I have looked into it very closely indeed, and I find it is entirely due to the chlorine in the salt water.* There is no proposal here to to use the boilers with salt water in them, and consequently I see no reason for the distrust of the peroxide, which has already answered by actual practice over a very considerable time. A boiler in use for the last thirteen years, much longer I suppose than any other similar boiler has existed in the world, has resulted in a perfectly correct effect, that is to say, a boiler cut open after thirteen years' trial does show the peroxide, and does not show any pitting or any decay. Anybody can see who will take the trouble to go and look at them. Now, with regard to the cost of the engines. I think Mr. Barnaby has calculated the indicated horse-power as if it were produced in the ordinary way, and calculated his engines at a little too much per horse-power on that account. I think similarly with regard to the armour which he has spoken of. He cannot have considered how the armour is disposed as part of the hull. As to the cost of the whole ship, I am free to confess that I could not have said that this vessel could be built for the same cost as former vessels carrying fewer guns unless the price of construction had considerably reduced itself. In private yards I was informed yesterday that vessels were offered with engines at £14 10s. a ton,† which is a magnificent bearing for those who have not yet invested. The comparison of the lightness of the *Italia* is scarcely a fair one, because she has a peculiar construction of hull. She has a large number of armour-plated decks, giving a very great weight which is comprised in what is called the unarmoured hull, but which cannot be correctly so described. I think that the calculations will be found not to be so much out of the way as you might be led to think, and that is very likely, because there is here not one pound of iron used which does not contribute to cubical contents. There is no dead wood, there is no fine bow, no rudder and no screw; the weight is all applied

* On consulting Professor Barff I find that he did not supply the plates experimented on by Mr. Barnaby, and therefore cannot guarantee them, but is ready to guarantee that plates prepared by him will not fail in the same way.

† Now, in the Northern yards, £10 per ton of dead weight carried, for ship and engines complete.

so as to give space, and therefore cubical contents. Captain Dawson asked what was the meaning of those lines. As the construction I adopted was that of two segments of very large spheres I showed in the dotted lines the continuation of those segments up to the water-line, in order to give an idea of what the midship section would be if she were seen end on. The dotted lines also refer to those two great segments of spheres, which, being separated by a space in the centre, give the real shape of the ship. I will just say that I do not think you will find any lines in any existing type of ships so fine as those are, taking them all through. Now, with regard to the question of propulsion, I went very carefully through that matter, and I fortified myself first by taking the most recent experiences with regard to the driving of ships of light draught. I find that in America eight indicated horse-power per foot of midship section sufficed to drive a light-draught flat bottomed American steamer with large paddle engines twenty miles an hour. With regard to centre-board vessels they are exactly the shape of a spoon. Put a spoon in the water—that is a lengthened spoon —and you have a centre-board vessel. In the countries where deep-board vessels are used, no centre-board yacht is allowed to sail in any match where deep-keeled vessels are entered, simply because she can run away from all of them, off a wind, or on a wind, when she lowers her keel centre board. The analogy leads me to think that there is much more resistance due than is generally supposed to the water-column—the pressure. Now, I did not say that this same improvement in boilers and engines would not—on the contrary, I did say it would—double the efficiency of all existing ships. I hope it may do so, and I hope after trying those old shapes we may be led to consider something about the new. I am here really drawing attention to the fact that with a given size of ship, at a given cost, you could carry about four times the gun power which you now carry. It would be possible to do it. To call gun-pits turrets is to make a mistake; they are not turrets. The platforms on which the guns revolve are contained in the gun-pits, but the whole system of Moncrieff for raising guns above the level to fire them must be understood in its application to ships, before it can be compared with turrets, which have nothing to do with the question. Now, as to the *Popoffka*, I do not think that any beautiful idea, such as Mr. John Elder's indubitably was, because it was proved and carried out, could possibly be better spoiled than by the *Popoffka*. Admiral Popoff chose to take off the fine lines at the bar and all round, in fact, the whole of Mr. Elder's fine entrance and run, and substituted a bow very nearly upright to drive against the water. I can easily conceive that that would produce a very heavy resistance indeed; but when you come to consider that the alteration of the swim in a canal-boat immediately leads to the same result, I do not think we need wonder that she cannot be driven fast in a sea-way, and that she will not behave well in a sea-way. That is a matter on which I must differ, as a seaman, from those who have spoken on the subject. Mr. Scott Russell asked me why I chose two turrets, or two gun-pits, and two spheres. For this reason, because, first of all, the greatest fault of the *Popoffkas* is that they will not steer under difficulties. They have no inclination to go one way more than another, being circular, and not having Mr. Elder's turbine propeller. Secondly, it was necessary in this, as in all other naval structures, to provide for damage and accident, and that is provided for by duplicating the power. Thirdly, because the same thing occurs with gun-pits. You may get, although it is not probable that you will get, an unlucky shot, or some accident, and it is also wise to duplicate your powers of offence as well as those of propulsion. The length also gives me what I could not gain in any other way, and what, I think Mr. Scott Russell will allow, with his great knowledge of wave-form, is very valuable, that is to say, a means of dividing rather more quickly the water, than the water can re-enter. The whole shape is very much that, in fact, of an ordinary aquatic bird's under-water section. It is very nearly the same, and I think it would be found in many ways to produce the same results. But what I draw attention to here, and what I have come to-day to show is, that there are possibly certain

things that could be done. I did not advocate this particular way of doing it, but I advocate investigation into what are confessedly novelties, because I think this Institution, however great its work may be, in appreciating that which has been done, has had a very large portion, and a very valuable portion of its work directed to the advocation, and the pressing on for adoption, by experiment at least, of very great novelties ; and I think the members of the Institution would regret to see the day when novelties were not received at least with that fair examination which their merit may call for. In justification of what I believe as to the merits of the hydraulic propeller, I beg to add to my paper a pamphlet (now out of print I imagine) by the late Wm. Gravatt, C.E., F.R.S., on gunboats of light draught and high speed. This will I am certain, give to whatever I have been able to advance a weight and value which no observations of my own could possibly have had without it, particularly as experiments have been made lately which conclusivsly prove his reasoning to be correct as to the value of the reactive force due to a given head of water.

ON A SYSTEM OF MECHANICAL SHIPBUILDING.

By J. Humphrys, Esq., Member.

[Read at the Twentieth Session of the Institution of Naval Architects, 5th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

I have endeavoured to make the paper I am about to read to you as brief as possible, trusting that the models and plans I have laid before you will supplement my description, so as to render my meaning quite intelligible.

Ever since I have been connected with shipbuilding, I have been very much impressed with the peculiar feature which attaches to it, viz., that the builder should be almost entirely dependent on skilled manual labour, and whereas we find in most other important manufactures that there has been a constant aim to minimise skilled manual labour, and cheapen and improve the workmanship by the introduction of mechanical appliances, little, so far as I know, has been done in this direction as regards shipbuilding.

The reason, I think, is obvious. Iron shipbuilding grew directly from wooden shipbuilding, the methods of construction being very similar; in both there is the skeleton framework, consisting of keel, keelsons, frames, stringers, beams, &c., on which is laid the skin, in the one consisting of wooden planking, and in the other of plating. This has been the received mode from time immemorial, and is, as I may say, grafted into the nature of all naval architects, and from its very arrangement it is almost impossible to bring to bear any mechanical contrivances to aid the builder, such as are met with in almost all other important manufactures.

Now to trace very cursorily the way in which an ordinary iron ship is constructed, we find that the designer's lines are projected to the natural size on a floor, that the angle irons forming the frames and reverse frames are placed into long furnaces, heated, and bent by skilful workmen to these lines; the floor-plates and beams are similarly dealt with; these parts are then taken to the place in the building-yard where the vessel is to be erected, they are fastened together, usually by hand riveting, are then erected on loose wooden blocks fixed in position, and shored by means of rough poles, ribbands, and stages; the skeleton is then faired and tied together with the keelsons and stringers, and lined off for the skin plating, all this work being dependent on the skill of shipwrights, and requiring constant care and adjustment to counteract subsidence and keep the structure fair; then comes the plater with his wooden templates, which are marked off from the skeleton and transferred to the plates,

which are marked and afterwards punched (the accuracy of every hole being dependent on the skill with which the plater and his assistants can manipulate the plates under the punching-press); the plates are next set, and then hung on the skeleton, and the riveting is proceeded with, which is all effected by hand labour.

This method of construction has, I believe, hitherto defied all attempts to bring mechanical appliances to bear satisfactorily except to a very limited extent, such as, for instance, the riveting of the frames, floors, and beams by mechanical riveters.

Shipbuilding is, therefore, perhaps more than any other manufacture, except the building trade, placed at a disadvantage in relation with its workpeople, and this may be the reason why we find, I believe, a greater number of labour difficulties in these trades than in any other.

When we consider the vast importance of the shipbuilding trade, and the enormous sums which are annually expended in this country for the building of ships, you will, I think, admit that it is a matter of the first importance that, if practicable, shipbuilding should be to a much greater extent than at present assisted by appliances, and so be placed on a footing somewhat similar to that of, say, marine engine or locomotive building.

It occurred to me, therefore, that as the present mode of construction appears to present insuperable difficulties in this direction, it might perhaps be possible to devise a novel method of construction by which, in a great measure, the dependence of shipbuilders on skilled manual labour might be lessened; and what I propose is a longitudinal mode of construction (which is generally admitted to be stronger than the present ordinary mode, but hitherto being considered too costly it has seldom been adopted), the material to be used to consist of channel sections, the flanges and other parts being so arranged that the riveting which connects the fabric together may be readily effected by mechanical riveters.

I propose that the process of building shall be carried out as follows. A permanent building-shop consisting of strong and rigid frames erected at regular intervals in the direction of its length, and braced together longitudinally, covered by a suitable roof, and somewhat in accordance with the plan exhibited, is to be built on a firm foundation, the whole forming a perfectly rigid framework capable of sustaining the weight of the ship. In these frames I would fix suitable packings made up to the exact transverse sections of the ship at the various stations, and on these packings I would inscribe offsets representing the points where the connecting flanges would meet each other: the particulars could be readily obtained by projection on the mould floor. A skeleton mould or cradle representing the form and lines of the vessel would thus be formed in which the ship could be built without risk of subsidence or loss of form.

A system of cranes could be conveniently arranged above, from which the mechanical riveters could be carried (and if on the hydraulic principle they could be in communication with a hydraulic main and accumulator for working them).

I propose that there should be a number of the channel sections varying in sizes and thicknesses, and which might be described as No. 1, No. 2, No. 3, &c., &c. A code of rules could be framed similar to Lloyd's tables, from which could be ascertained the weights of the

various sections required in the construction of any given size of vessel, such size to be based, say, on the under deck tonnage, &c., &c.

This would offer many advantages. The shipbuilder would be enabled to order the material required for a ship immediately on receipt of an order, by referring to the table (it takes from two to four weeks under the present system to do this), and there would be little or no waste in the form of scrap as against a loss of 5 to 7 per cent. as at present. The maker would merely have to roll the sections and cut them to fair lengths (just as in rolling rails); he could also roll to stock, and so afford to put in the rolls for any section at any time, by which also a considerable saving of time would be effected; and considering the large quantities that would be required for shipbuilding, I have reason to believe, from communications on the subject with ironmasters, that these sections could be produced as cheaply as the materials used in the present mode of shipbuilding.

The sections on delivery at the shipbuilder's works could be at once passed through self-acting punching machines, the holes made with the utmost rapidity, all holes being at a uniform distance from the edge, and precisely equi-distant from each other, and the distance between the planed butt and the centre of the last hole being exactly half the distance between the centres of two holes, all the holes when the sections were fitted together would be absolutely fair, and the rivets being fastened by hydraulic or other approved mechanical riveters would exactly fill the holes, and so make the soundest work. For a considerable portion of the ship the sections would require no setting whatever, as they possess sufficient elasticity to admit of a considerable amount of twist or "sny," which would be obtained simply by securing them in the cradle. The number of drop plates, and those requiring special working, is but small, as may be seen from the models, and from the experiments I have made I am convinced there would be no difficulty in dealing with them in a thoroughly satisfactory manner.

The sections, as prepared, would then be laid in the cradle, their edges being placed so as to coincide with the offsets inscribed on the transverse sections, as described previously; the keelsons would be placed in position, and the mechanical riveters could be at once applied and the connection of the fabric proceeded with; the structure being built up from the bottom and completed as the work advanced, so effecting a very considerable saving in time as compared with the present system, by enabling other trades to follow more rapidly. A water-tight double bottom could be very readily formed by the proposed system, and would be, I think, far more reliable than the water ballast bottoms as at present constructed.

The cost of labour would be very greatly reduced, as the furnacing and setting of frames, reverse frames, and floors would be dispensed with, and a great saving would be obtained in a large portion of platers' work, particularly punching, templating, and setting; and of shipwrights' work in shoring, staging, and ribbanding, all risk of subsidence and loss of form (necessitating at present constant care and attention) being obviated by building in the rigid cradle; and sound and cheap mechanical riveting would be substituted for expensive hand riveting, which I fear is not always so faithfully done as it should be.

I think there would be also a decided advantage to the manufacturer of the material by

its being confined to a certain number of recognised sections, and that they could be rolled much as rails are. All these considerations would result in a great rapidity of construction, which in a large vessel might amount to two or three months' less time than at present required.

I have had two midship sections prepared (see Plate XXIII.), one being that of the steamers built by the Barrow Shipbuilding Company for the Ducal Line, which are of 3,000 tons gross register, and 380 feet between the perpendiculars, 38·3 feet beam extreme, and 28·6 feet depth in hold; the other of a vessel of the same dimensions to be constructed on the channel section system. I have made the thickness of the skin in the latter the same as that in the Ducal steamers, and in both cases only the hull proper is dealt with, irrespective of deck houses, &c.; the weights have been most carefully calculated from this midship section, and amount to 1,390 tons, whereas the weight used in the Ducal steamers was 1,440 tons, showing a small reduction in weight, but by the channel section principle a complete and water-tight double bottom is obtained, which does not exist in the other, and the fabric is constructed on a longitudinal system, which I am convinced is far stronger than the existing mode on which the Ducal steamers were built.

In the very important matter of repairs, also, the proposed system offers some advantages, as every vessel so constructed might always carry with her a few plates, which being punched to a uniform gauge could be readily fixed to replace a damaged plate or plates, the only labour required being to cut out the defective part, put in a spare plate or plates, and rivet together, platers not being required.

I have had this matter under careful consideration for nearly two years, and feel convinced the proposed system of construction will afford many and great advantages to all concerned in shipbuilding; and although doubtless, as in all new things, some unforeseen difficulties may arise, I do not think they will prove such as may not readily be overcome, and that they will be as nothing when compared with the many advantages presented, which offer, especially from a commercial point of view, such great desiderata; and if the system be adopted, I have little doubt that the skill and experience which others may bring to bear on it, will rapidly lead to improvements and refinements, and that a fabric so constructed may become as superior to that I have ventured to lay before you as the present iron ship is to those of earlier types.

DISCUSSION.

Mr. Wm. Denny: My Lord, I think this Institution ought to thank any gentleman who, like Mr. Humphrys, tries to bring before us a new method of construction, especially when he tries to do it in the way in which Mr. Humphrys has done this. There is no doubt whatever that one must look at these questions both from the point of view of the efficiency of construction and the economy of labour, and Mr. Humphrys has so far attempted to solve the problem. But, my Lord, if I ask the Institution to recognise this, I trust they will bear with me in pointing out that I do not think the invention altogether fulfils the inventor's expectations. First, I would wish to call

your attention to the fact that Mr. Humphrys has here brought before us a very valuable thing, that is, the new system of hydraulic riveting, which is becoming applicable to all work which can be got at from two sides. But Mr. Humphrys starts with the assumption that there can be no other machine riveting applied to the ordinary iron ship. Mr. Humphrys, I think, must be ignorant of the fact that a gentleman who is in this room, and whom I see sitting before me, has attempted to carry out a system of machine riveting upon the sides of the ordinary iron ship percussively by means of steam riveters. I refer to Mr. McMillan, of Dumbarton. I believe that the scheme has all the elements of success in it, if it were not for two defects. One defect is that the workmen, as you might expect, set their faces against working it efficiently, and working it in the only possibly efficient way, upon the piece-work system. The other defect arises from the fact that in working with this heated apparatus, the men never have the comfort which they have in working with a cold apparatus like that which Mr. Humphrys has brought before us. But with regard to both the workmen and the heat, I believe the difficulty can be got over. I understand that there is a French firm in Paris, who have applied a compressed air riveter to the same purposes, and very likely that will solve the difficulty, so that we must not start with the idea that the days of the old construction are finished, because mechanical power cannot be applied to rivet the skin. Even granting, however, that this would be a very great saving, I must point out to you that, counting the number of streaks round that section of Mr. Humphrys, I find from the keel to the gunwale there are thirty streaks. In an ordinary ship, we should not require more than sixteen streaks. Now, to begin with, here is practically double the amount of riveting, which even if done in a more economical way would require to be done to four times the efficiency, before you would have half the economy. You would not only have double the amount of riveting, but double the amount of caulking. Then there is another point. I think that for working this system of construction a finer quality of iron would have to be employed than Mr. Humphrys anticipates. We heard yesterday from Mr. Samuda much about the necessity for using a fine quality of iron, but this may be overstated, for I do not know that there is an instance on record of any ship that has been lost simply and purely from the bad quality of her iron. If there are instances they must be very few. Therefore, granting that an ordinary ship can be built with ordinary iron, this is a difficulty that would arise in this case. There are several features in this scheme which I think are worthy of attention, apart altogether from the difficulty of carrying it out. Mr. Humphrys has recognised what I call massed transverse framing, and he has recognised in that a most important thing, but it is not necessary to have this construction to carry it out, because we have been carrying out massed transverse framing for the last two years, as Mr. John knows, in the ordinary construction. There is another thing which Mr. Humphrys has done. He has not only massed transverse framing, but he has what we might call massed pillaring, which is a very great advantage, because by having the pillars few and far between, you are enabled to make them a great deal heavier; they do better work, and stand the packing up of cargo against them much better. Then Mr. Humphrys has done another thing: he has made the double bottom in this ship a constructional part of the work; but this can be done with the present construction, and in the case of six steamers, which we have built of iron, the double bottom is a constructional part of the work, going from end to end of the steamer, and not only acting as a double bottom in the way of ballasting the ship, but also in the way of strengthening her and making her safe. I would ask Mr. Barnaby whether it would not be fair, among all the consideration which he is giving to merchant ships and their requirements, to add to those requirements that of continuous double bottoms from end to end as being in every way an advantage to the ships. I can assure you that the double bottom, which we have carried out in this way, is not only one which fulfils all the requirements which Mr. Humphrys

seems to think are only fulfilled by this double bottom, but is one which is capable of being easily and thoroughly inspected and cleaned. It slightly resembles the Admiralty system of construction, and is a development of a double bottom which was brought before this Institution by Mr. Martell, in Glasgow.

Mr. J. D'Aguilar Samuda, M.P.: My Lord, I agree with Mr. Denny that there is a great deal of ingenuity in this plan. Of course there would be difficulties in introducing any new plan, and no doubt there are some points that Mr. Humphrys might advantageously explain to us how he manages to meet. The general arrangement, so long as the ship takes a parallel form, is very simple, but when he has to deviate from that parallel form, and to get his plates tapering, it appears to me that he has not yet explained to us the course he intends to adopt with the view of making those plates fit in accurately; whether he proposed to have them all rolled to the taper which his streaks were compelled to form, or whether he proposed to have sudden breaks, and to take it up in a somewhat similar way as a new construction of the ship. That does appear to me to be a matter of much more importance than it would at first sight show itself to be, because in ordinary vessels, I suppose, after having once got away from the first 80 feet of the midship section, no two plates could be rolled except they were rolled specially by a variation equal to the amount of difference of width that he required to exist in the one end of the plate from the other. I am now suggesting some of the objections which appear to what I think otherwise is an extremely ingenious plan. Another objection, of course, is in the extremely expensive arrangement of mould which it is necessary to have, according to Mr. Humphrys' view, for building the ship with. But I agree with Mr. Denny that mechanical riveting does not owe its entire adoption to a system of this sort. I have myself seen several plans in which mechanical riveting has been suggested to be used for the present mode of building, but I must in justice say that it has not yet made its way sufficiently to make it one of general application in the trade, and therefore the difficulties of it at the present moment may fairly be met by suggesting a better mode of introducing it, as this appears to do. There is a great objection to this, of course, in the extra quantity of riveting that would result from putting the seams so much closer together, but then, on the other hand, I do not think Mr. Denny made quite sufficient allowance for the saving that would accrue from doing away with the entire riveting of the frames, both to the reverse frames and also to the skin of the ship, and perhaps therefore there is not much difference after all in riveting required in this form, and that which would occur in the ordinary construction. I was a little amused by the clever way in which Mr. Denny suggested that an objection would arise in this vessel from the requirement of superior iron. To me that is one of the greatest recommendations that this scheme possesses. I think that whatever information we may possess, and it may be small, as to the number of ships lost from bad iron, if the ships that have gone to the bottom could speak, you would find that the bad iron has got a great deal to do with it. I believe nothing has done shipbuilding so much mischief, and continues every day to do it, as the curse of bad iron, and the greater curse of people being able to insure against loss. Until we get a change by which it is made penal for a man to insure a ship up to all he can lose by her foundering, we shall have ships constructed of bad iron, and shall never have them made of such iron and of such manufacture as they ought to be. I do not think it becomes a part of our business to enter upon this now, but it is a theme which might well exercise the best ability of every one of those I see around me, who are so largely engaged in shipbuilding, to endeavour by their influence to produce, on the country, the conviction that those two matters lie at the root of all the misfortunes to ships, and a great deal of the depression of trade existing at the present moment. On the whole, I am very favourably disposed to this matter. I do not think it will be one of easy introduction. I think it is one that will require all the energy of a man like Mr. Humphrys to carry on successfully, and it will be some time before he will get us to adopt it as a general rule, because, I

must say frankly, although it is extremely ingenious, and although there are many points, particularly the nice arrangement for carrying out double bottoms, yet I think it has not sufficient advantages over the ordinary plan to make it so patent that people will be compelled, as it were, to accept the one and discard the other. It only becomes a question of the balance of advantages, and people will only accept it after they have the means of seeing it practically put into operation by a man who is devoting his whole attention, to getting over the enormous difficulties that he will have, before he will be able to make it perfect. On the whole, I think it is a most interesting paper, and that we ought to be much obliged to Mr. Humphrys for bringing it before us.

Mr. Denny: Will your Lordship permit me one word of explanation with regard to what Mr. Samuda said. I think he has perhaps taken me up on the impression that I advocate the use of bad iron. I do not: nothing of the kind. But there are different qualities of iron, and there are plenty of qualities of iron quite good enough for shipbuilding, remarkably good iron, but which could not flange in the way in which the iron there would be required to flange. I think if this Institution were to recognise the idea that ships should be built of best iron, it would be making an entire mistake. Mr. Samuda has referred to the depression of trade. I have no hesitation in saying if his views were adopted that the cost of ships would be so much that the depression would be greater.

Mr. Samuda: I do not hesitate to say that the skin of every ship ought to be built of best iron, and nothing short of best boiler plate ought ever to be used.

Mr. Denny: I do not think it is necessary.

Mr. W. John: I agree very much with the remarks which have fallen from Mr. Denny in reference to this mode of construction and the difficulties that would arise from it. I think they are chiefly connected with the ends of the ships, otherwise the method of construction is a simple one, and offers many facilities. Of course, with regard to the question of material, it would have to be either steel or a very special kind of iron, and on this question of iron I should like to say a word. Mr. Samuda has said a good deal about the bad iron used in ship construction. Now I think I may say that the surveyors of Lloyd's Register have had as good opportunities of knowing the qualities of iron used throughout the country as Mr. Samuda or any one in the country, and we have had experiments made over and over again where we have had suspicions of bad iron, and I will venture to say that the iron used throughout the country for shipbuilding is not of the character described by Mr. Samuda, but it is a good, substantial, strong, ductile material. It is a material that will stand 20 tons to a square inch, and if you were to go through the shipbuilding yards right round from the east coast to the west and take samples, you would find almost invariably that iron would stand tensile strains of 20 tons to the inch, and bending and ductile strains equal to the Admiralty tests for best iron. Therefore there is but the remotest chance of ships being lost from bad iron. Mr. Samuda implied yesterday that Lloyd's rules were framed to meet the scantlings that would be required supposing the iron was bad. I say they are nothing of the kind. They are framed upon a good uniform quality of iron, and it is the duty of the surveyors to our Society to insist upon the iron being good, and they do so, and they get good iron. It is all very well for a builder to stand up and talk about using Staffordshire iron, while his fellow-shipbuilders are using pot metal, but that sort of thing is not the case. I believe there is more uniformity in the quality of material used throughout different building yards in the country than there has ever been before. Then with regard to the question of steel. It has been said that the Committee of Lloyd's have hampered the introduction of steel. I say instead of doing that they have done everything that has been prudent to facilitate the introduction of steel—this mild steel that is suitable for shipbuilding. They sent round their surveyors, who have been to the manufacturers; tested the material in all quarters, consulted with the builders, and they have most carefully considered what reduction from iron should be given; but Mr. Samuda comes here without knowing what has been done, he does not

remember even the fraction allowed off iron, but he thinks it is a fourth, and he condemns the action of Lloyd's positively without knowing what they are doing. Then there is another point, my Lord, to which I would refer, and that is the position we take with regard to novelties of this kind. Mr. Samuda says, Let Lloyd's point out defective workmanship, and do not let them interfere with the freedom of the shipbuilder, and tell him what he ought to do. It is the pleasure of the Committee of Lloyd's, and all the staff of Lloyd's, to go out of their way if they can to assist builders in developing new systems of construction—in not keeping them to the mere hard-and-fast requirements of the rules, but if they can go out of their way to assist Mr. Humphrys for instance, they would do so, as they did in the construction, as I pointed out yesterday, of that peculiar novelty, the twin-ship. We there did all we could to assist the builders to develop the system of construction, and if Mr. Samuda had more acquaintance with the present working of Lloyd's he would never have made the remarks he did yesterday. I say that Mr. Samuda, when he did build merchant ships, some years ago, used iron the same as other people used, and the surveyors to our Society had as much trouble with Mr. Samuda's iron as they had with other builder's in the country.

Mr. Samuda: Really, my Lord, I must call Mr. John to order. I should like to know upon what authority Mr. John ventures to say that any iron that ever I used was of an unsuitable description to put into a vessel, or that his people had any trouble with any iron which I produced. I think it is a most improper statement to make, and I defy him to prove it. I insist upon Mr. John withdrawing those words which imply that I have put into my ships iron of an inferior quality to that which I have described as necessary for them. I insist upon that being withdrawn, my Lord.

Mr. John: My Lord, I have no wish to imply anything of the sort.

The President: One moment, if you please. Mr. Samuda has made an appeal to me which I must say under the circumstances I think is a very natural appeal. My answer to it is merely this. We have always conducted our discussions here in so good a spirit, and with a desire on all sides to conduce to useful ends, that I am sure Mr. John will be ready to withdraw anything that could have fairly hurt the feelings of Mr. Samuda.

Mr. John: My Lord, if I said (I do not think I did) that Mr. Samuda had put iron into his ships that was unfit to go into his ships, I will withdraw the expression. I will not say what I meant to say.

Mr. Morgan: My Lord, I must rise to order.

The President: There is no question now before the meeting. Mr. Samuda has made an appeal to me. Mr. John has met that appeal, I think very fairly, and in a manner which I hope is satisfactory to Mr. Samuda. Therefore I think we had better continue our discussion now.

Mr. John: I will only say one word. It is farthest from my wish at any time to enter into what may appear to be personal questions, or to say harsh things, but I think I have not been without provocation on this occasion, and Mr. Samuda has himself to thank for the tone the discussion has taken.

Mr. Samuda: Order, order.

Mr. J. Scott Russell: I think that is a renewal of the imputation.

Mr. John: Oh no.

Mr. Scott Russell: It appears to me to be a renewal of the imputation.

Mr. John: If your Lordship thinks so I will withdraw the words.

Mr. W. H. White: My Lord, may I be permitted to say one or two things about Mr. Humphrys' system of construction, which I have studied with considerable interest? Of course it is not a matter on which I can speak with certainty without calculation, but I should think it probable that, adopting so great a spacing of partial bulkheads, Mr. Humphrys has in his mind the use of a

considerable number of complete transverse water-tight bulkheads in the vessel of the proportions stated in his paper. Of course the use of widely-spaced partial bulkheads has been made common, by Mr. Scott Russell's influence I believe chiefly, but it does seem to me that for a vessel of this depth, unless she had a number of complete tranverse water-tight bulkheads, or very strong partial bulkheads at comparatively frequent intervals, longitudinals of the scantlings shown on the model would hardly do their work. I ask for information. I presume also that at the ends of the ship Mr. Humphrys would introduce a series of stiffeners of a transverse vertical character to help the longitudinals to do their work against the panting strains, which frequently cause damage in badly-built ships.

Mr. H. Edwards: My Lord, when I looked at the model I thought it was an adaptation of my own, but I find it is not. I wish to ask the writer, Mr. Humphrys, the question, how long it is since that idea had struck him? Mr. Denny, I think, made a remark about the narrow streaks on the section, and he said that he could build a ship with half the number of streaks. Now that is one complaint that I have to make of iron shipbuilders, having wide streaks. Very few men have had more experience of ships than myself. I have seen ships coming into dry dock with nine to thirteen streaks taking the whole girth amidships, some of those streaks four feet six inches wide. You may go round that ship, and Mr. John may say what he likes about Lloyd's and the surveyors who examine them, and you will not find but what there is a move in the butt action, in my opinion, by these broad streaks. I think Mr. Humphrys has got quite the idea in this longitudinal system of framing, but my system is, instead of the plates being flanged to have a T angle iron riveted on the joint of each plate, which I asked Mr. Samuda to look at once, and it is a system which I should like Mr. Scott Russell to see. The frame runs fore and aft with a T iron. It is placed on the joint of each plate. The riveting is single riveting. The transverse riveting I throw away entirely. It has been before the Admiralty, it has been before Lloyd's, but unfortunately I am a poor man and cannot bring it out. I must get some of you gentlemen to look at it by and by. It is undoubtedly a cheaper system; it is a lighter system; it is a safer system, and a stronger system than that which we have before us. It is lighter, because I save the laps of the plates, I save the short pieces of the angle iron to connect the intercostals. I save the lining piece behind the outside plate, and I should say I save something like 10 per cent. of weight without the least piece of iron being less than what Lloyd's require in any ship they like to mention. The larger the ship the better my system applies.

Mr. J. Scott Russell, F.R.S.: My Lord, I could not allow this paper to pass without recording my approbation of a great deal of it. A great many years ago, before I introduced what they call the longitudinal system of construction in my large vessels, I thoroughly considered this very point, and I think the inventor of it will admit frankly that at that time, and in that stage of our iron manufacture, some twenty or thirty years ago, we should not have even dreamt of having those very pretty plates which he has given us. I assume from his position as a practical man that he has already ascertained that he can have those plates absolutely perfectly made of extremely good iron, and at a moderate price: I assume that as a practical man he has done that. That gets over a good many difficulties. Now he must have some good reason, which he has not told us, why he makes these so very long and so very narrow instead of a little wider. It pleases our friend no doubt very much that it should be so narrow, but it displeases me, because I should like to have one set of rivets doing the work of two sets, and I am not an objector to a broad streak—I rather like a broad streak.

Mr. Edwards: On the deck.

Mr. J. Scott Russell: I like a broad streak of iron. If it is, say, half an inch thick, I do not like it to be much less than thirty inches wide. If it is three-quarters of an inch thick, then I should like my streak to be about thirty-six inches from centre of rivet to rivet, and thirty-nine inches over

all. That would of course economise this in some respects, and it might make difficulties, I suppose, as to the rolling of the streaks. All that, I hope, the inventor will show us he has got rid of, in some way or other. I think we must be unanimous in saying that the doing the whole midship parallel body of the vessel in this manner presents hardly any structural difficulty. I think also that it is very convenient for the construction of an iron bottom. I should not wonder if in the end we find that, the most convenient manner of putting together an iron bottom. That I do not know, but this I do know, that I worked the whole of this out at one time, and excepting that our manufacturers were not up to it, I liked it very much. I do not know that attention has been called to the great beauty (and there are a great many advantages along with the beautiful) of the uniform skin we have got outside. I presume the rolling and arrangement of the riveting we do not yet quite understand. I know the great difficulty I had in inventing an absolute continuous system of riveting, which I did in my vessels, and in the *Great Eastern* an absolutely perfectly mathematical system, all the rivets the same distance from each other, and all the plates fitted the moment they went together without any adjustment whatever; that is to say, a plate came from *there* and another from *there*—two plates that had never seen each other before—and they all fitted together absolutely perfectly in their places. The great advantage of that is, that you do the thing in once instead of doing it in twice, and a perfect fit is always much better, and to my mind much cheaper, than when two things fit nearly and have to be adjusted afterwards. I daresay the inventor has provided for that. There is the remaining difficulty which Mr. Samuda called attention to. I think the author has himself got at the difficulty, although he has not explained it to us. In developing an absolutely mathematical system for the *Great Eastern*, I had to get over the same difficulty which he had here when he leaves the middle body. When he leaves the middle body, what is he to do? how is he to get his tapers? how is he to get his double curvature? how is he to make his mathematical edges and flanges? That is extremely difficult. Now the way I got over all that trouble in the *Great Eastern* was this, and you will be horrified when I tell you. I left every alternate streak of the ship its full parallel breadth from end to end of the ship. I say I left every alternate streak an equal breadth from end to end of the ship, only that gave me the trouble of calculating the dimensions in every alternate plate. Now something like that appears here, and if you have the general design of the ship, I daresay you would contrive the ship so as to have a great number of parallel plates running all through, and then concentrating the exceptional diverging or tapered plates at certain places. I observe in this instance that they are so concentrated. Permit me, in fine, to say that these ships require much more skill in the designer, and much greater forethought in the putting together, than our ordinary construction would have, but I am very hopeful that with skilful designers and constructors, this may be found a very efficient and very durable, and, in the end, an economical structure for a ship.

Mr. C. W. Merrifield, F.R.S.: My Lord, there is one question I should like to put to the author. I have not had the advantage of hearing the whole of the discussion, but I have looked at the paper, and there was this difficulty that presented itself with regard to a scheme of shipbuilding not unlike this in some respects, where the arrangement of the plates was transverse instead of longitudinal, which was presented before this Institution some year or two ago, that is to say, what security is there that the butts will not open under tension, and destroy the fairness of the surface of the ship when exposed to the ordinary racking strains arising both from the sea and the loading?

Mr. J. McMillan, Junr.: My Lord, Mr. Denny has referred to our scheme of skin riveting. About three years ago we introduced a skin riveter, and since then have continued using it on the shells of our ships. We find the work to be very good indeed, Lloyd's having also approved of it. To show the amount of work one of these machines is capable of doing, on one occasion we put on a very good man. In half an hour he put down 60 rivets. In time-work as a rule I believe the average put

down by two riveters, one holder-on and a boy, is something like perhaps 140 ¾-inch rivets, and on piece-work the same men could put down 250 in a similar time. Working this steam riveter as we have done it hitherto our men have not taken to it, but notwithstanding that, we continue using it, and our present system is to pay them a day's work if they put down 140 rivets—we give them a bonus for anything over that. When they take to it I have not the slightest doubt that the steam riveter will be taken up and highly approved of by shipbuilders who do not now use it.

Mr. J. Humphrys: My Lord, with reference to the observations Mr. Denny made, I wish to point out that as to the hydraulic riveter I do not speak of the hydraulic riveter *par excellence*, by any means. I simply say that I speak of mechanical appliances, and I merely illustrated the hydraulic riveter as a very handy mode of mechanical riveting, and such indeed as has been very universally adopted. If you come to compare the riveters together, I think I am not very far wrong if I say that steam riveters were generally used some years ago for boiler making, and were abandoned for hydraulic riveters. I was partially aware of Mr. McMillan's very ingenious contrivance for riveting the shells of vessels, but I was rather surprised to hear that it was in general use, as he now states. With reference to the number of streaks in the proposed vessel being greater than they would be in the ordinary mode, and therefore that the riveting would be greatly increased, I wish to point out that in most vessels the riveting is very commonly what we call double chain riveting, and that by the system I propose to adopt here my intention was to have only a single row of riveting, so that practically the number used would not be much different from that used in the present system. Then with regard to the quality of the iron, all I can say is, I put myself into communication with some of the first iron-masters in the kingdom before I would go on working up this notion, because everything hinged on that. If these sections could not be rolled at a marketable price, then there was no need to do the whole thing at once. But some of the most eminent iron-masters informed me that if this became a system, and the material was produced in such large quantities as at present, they had no hesitation in saying, it could be produced at a cost about the same as the present ship-plate. I think that may be further exemplified when we find that girders, Belgian for instance, are even now delivered here in small quantities at about £7 per ton. Then with regard to the double bottom, that we know is no novelty at all—a longitudinal double bottom. I only say that this system offers a very ready mode of doing it. Then with reference to the question that was asked by Mr. Samuda, and also further adverted to by Mr. Scott Russell, as to the mode in which the tapering plates would be dealt with—that I got over very much in the way that Mr. Scott Russell has indicated. I run out from amidships as many streaks as I can cut parallel from end to end of the vessel. Occasionally I have to deal with what is commonly called a drop or sketch plate. If you observe the model here, and count the number of sections down, here, you will find the few sketch plates required are comparatively small as compared with the aggregate required for the entire sturcture, and there is a very simple mode indeed in which sketch plates can be made. All you have to do if you want to have a sketch plate is to split up a piece of the parallel stuff—cut off from the inside as much as is required, and then put the two together and rivet them, and then you have your sketch plate Then with reference to Mr. White's remarks, of course the transverse bulk-heads are intended to be introduced here which formed part of the weights when the calculations were taken for the purpose of comparison, and are introduced here. The arrangement for the panting both forward and aft are also introduced, and were allowed for in calculating the weights. They are not shown here, because we wished to show how the skin was dealt with there. I did not catch the name of the gentleman who called attention to the possibility of the work opening. I do not think we need apprehend much from that feature, inasmuch as we have here perfectly fair work; all the holes are absolutely true

with each other, and they are riveted together by a mechanical riveter which I think is more likely to hold the work firm and secure than the present mode adopted for hand-riveting.

Mr. Scott Russell: There is one question more. I should like to ask, why this particular breadth of plate was chosen.

Mr. Humphrys: For this reason, that in the interviews which I had with the iron-masters, they thought it would be advisable to limit it to 20 inches in width for the convenience of manufacture.

Mr. Scott Russell: For the present?

Mr. Humphrys: Yes.

Mr Scott Russell: I thought so.

LLOYD'S YACHT REGISTER.

By Dixon Kemp, Esq., Associate.

[Read at the Twentieth Session of the Institution of Naval Architects, 5th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

Yachts may be regarded as the production of the present century, as although it is on record that during the Tudor era vessels built for pleasure were described as "yachts," they were merely Government vessels, intended for the use of Royalty, and officers of State. At the commencement of the present century there were, perhaps, twenty yachts in existence, owned by private individuals, but their exploits were confined to the sheltered water inside the Isle of Wight, with Cowes for head-quarters, as yachting in the Channel might have been attended with inconvenience at that period. However, in spite of French cruisers and the confined limits of yachting a taste for the new pastime was rapidly developed, and in 1812 the fifty noblemen and gentlemen who then owned yachts formed themselves into a yacht club, which club has blossomed into the present Royal Yacht Squadron. From that period the number of yachts rapidly multiplied, and the fashion of yachting gradually pervaded all the moneyed classes. The great progress yachting has made during the last half-century can be gleaned from the following table, which shows the number of yachts there were above ten tons at different periods from 1812 to the present time :—

Year.	No. of Yachts.
1812	50
1850	400
1864	650
1878	1,400

Besides these, there are some five hundred yachts of ten tons and under, bringing the total up to nearly two thousand.

The early yachts were mostly built on the plan of the Revenue cruisers of the period, and the shipwrights' construction of these cruisers was generally followed. However, it was soon discovered that the amount of timber used in the construction of a Revenue cruiser was quite unnecessary for a yacht, and the scantlings got lighter and lighter, and some owners, who personally superintended the building of their yachts, dispensed with many of the through fastenings, and the longitudinal framing in the shape of limber strakes and bilge strakes. These, however, were exceptions in yacht construction. Yacht building had become a recognised branch of industry, and the builders, whilst not displaying much originality in

their designs, wisely built with sufficient regard to strength and durability. There is evidence existing of this, as one yacht built sixty years ago on the Colne* is at present cruising in the Mediterranean, and another,† fifty-four years old, is one of the most noted racing yachts of the period. A schooner fifty years old was recently bought for cruising in the Bosphorus, and besides these there are several yachts to be found in the "Yacht Register" whose ages range from thirty to fifty years. These facts can be accepted as evidence that the first yacht builders used good materials, and had an appropriate scheme of scantling and fastening.

As the number of yachts increased competition for their construction was extended. The immediate effect of this was that the builders' price per ton varied, and as a consequence the quality of the material and work varied also. At the same time another influence was at work which largely assisted in reducing the sizes of the scantling, even below what those owners had ventured upon to whom reference has already been made. Match sailing in 1851 became a pastime—nay, almost a rage—and for the sake of carrying a large quantity of ballast on any given displacement, the frames were made much lighter than was prudent, their spacing was increased, and the skin was dangerously reduced in thickness, and frequently only of soft wood. Beyond this, the sharpness of the form of the bottom which the exigencies of yacht racing had introduced almost precluded the use of grown floors, as they could not be found with sufficient dead rise, or if they could the throat-bolt had to follow the grain of the wood, which was highly objectionable. This led to the introduction of various plans of floor construction, some of which were expensive and decidedly advantageous, whilst others were cheap and weak. In fact, the greatest freedom was allowed builders in their systems of construction, and although some of them displayed considerable mechanical skill in obtaining strength, whilst reducing the weight of material, others showed very little of that skill, and many weakly-built vessels were the consequence. An occasional reference would be made to Lloyd's rules, but, as a rule, builders were apt to regard these rules with a kind of superstition, and appeared to believe that if any yacht were built up to them she would necessarily be nothing better than a merchantman, both in looks and in sailing qualities. Occasionally, during the last twenty years, a yacht was built under Lloyd's, and actually classed; but the total number of these classed wood yachts in 1877 had only been twenty. The owners' aim appeared to be to get a vessel built as cheaply as possible; the builders' aim, to use as little material, and that of as indifferent a quality, as his conscience or judgment permitted him to think would be necessary for the yacht to keep together for four or five years. Lloyd's rules were contemned for two urgent reasons: the first of these reasons the builder admitted to himself only, that is, by carrying the rules out to the letter he could not build a yacht without charging considerably more per ton—perhaps as much as thirty per cent.—than the owner would be pleased to pay. The second reason was told to the owner, and was to the effect that a vessel of very much heavier scantling than was used for a yacht, and one very much stronger than was necessary would be obtained if the rules were conformed to. The builder could appeal to his long experience in yacht building, and declare that he

* The *Pearl*, 160 tons, built by Sainty, of Wivenhoe, for the Marquis of Anglesey, in 1818.
† The *Arrow*, 117 tons, built by Inman, of Lymington, for the late Sir Joseph Weld, in 1822.

could in every way build a more satisfactory yacht according to his own rules than could be produced if he built according to Lloyd's rules. On the other hand, certain other builders built as closely to Lloyd's rules as possible, both in quality and quantity of materials, and adopted the system of fastening required by these rules. Still the fact remained that a large number of yachts were constructed in an unsatisfactory manner, and no one was more sensible of this than the builders themselves; and the wish was frequently expressed that Lloyd's would issue specially modified rules for the construction of yachts. But it must be confessed that the number of yachts that were annually built hardly justified Lloyd's moving in the matter, and, moreover, so far as known, those most concerned, the owners, had expressed no general wish for Lloyd's to interfere. However, the yacht navy gradually increased in number and importance, and in 1876 had reached proportions which justified a movement to place yacht construction under some such definite guidance as Lloyd's society exercised over the construction of merchant ships. Mr. John Harvey, the Wivenhoe yacht builder, was the originator of the movement, in this way. In November, 1876, he addressed a letter to me on yacht construction, in which occurred the following passage:—

"Lloyd's Registry of British and foreign built ships bears now, and has enjoyed for many years past, such world-wide reputation for integrity, that one feels as safe in the purchase of a vessel, or in the insurance or underwriting her, according to her standing on the list, as if every timber and fastening were open to inspection. Seeing that the general public are thus guardianed, we ask ourselves the question why any gentleman who is about to purchase or build a yacht should not have similar protection? Some agree that it would be better to build yachts in accordance with the directions given in Lloyd's book; but there are reasonable objections to this, as an unnecessarily heavy and costly structure would be produced; and thus seeing and admitting that the unsatisfactory system of building yachts under no special survey continues, I submit that the state of things points to the necessity of framing a set of rules to be observed in the building of yachts such as Lloyd's have framed for the construction of merchant vessels."

From such knowledge as I had of yacht construction, I could not but agree that Mr. Harvey's scheme was an excellent one, and one which ought to meet with favour. I advocated it warmly, and was fortunate in obtaining the co-operation of many well-known and influential yacht owners and yacht builders of high repute. A committee* was formed, and preliminary

* For the sake of completing the history of the foundation of the "Yacht Register" the names of this committee are herewith recorded:—

Marquis of Exeter.
Mr. Frank Willan.
Mr. G. C. Lampson.
Mr. Clement Millward, Q.C.
Capt. J. W. Hughes.
Count Batthyany.
Capt. Garrett, R.A.
Col. Angus Hall.
Mr. W. Baden-Powell.
Col. Dugmore.
Sir Richard Sutton.
Mr. W. B. Forwood.
Marquis of Ailsa.
Mr. W. Cuthbert Quilter.
Col. Gamble.
Mr. A. D. Macleay.
Mr. Benjamin Nicholson (*Messrs. Camper and Nicholson*).
Mr. John Harvey (*John Harvey, Ship and Yacht-building Company*).
Mr. Dixon Kemp.
Mr. J. A. Welch.

steps taken towards establishing a society similar to "Lloyd's Register of Shipping." When the scheme was made public, Lloyd's society, divining exactly what was required, came forward and offered to undertake the whole of the work that had been sketched out at the preliminary meetings of the "Yachting Lloyd's." This was at once recognised as a great advantage, as Lloyd's society had the machinery ready to hand for making surveys, and it was already foreseen that the proposed new society would experience some difficulty in obtaining and supporting duly qualified surveyors. Lloyd's adopted the scheme in its entirety; formed special rules for the building, equipment, and classing of wood, composite, and iron yachts; issued a special book containing these rules, which book contains a list of yachts and such particulars as it is necessary should be known concerning them. "Lloyd's Yacht Register" has now been in existence one year, and as evidence of its usefulness I may state that during that time over one hundred vessels have been classed, or about three times as many as have previously been classed throughout the whole term of yacht building.

The general body of yacht owners are perhaps still not thoroughly alive to the advantages of building under Lloyd's, or are unacquainted with the disadvantages or dangers of not doing so. A yacht to them is just the same thing, and still a yacht, whether she be classed or unclassed; and as yachting or cruising is usually confined to day passages from one port to the next port on the coast, it is not likely that any serious accident will occur to the general run of yachts that would convince owners that it would be an advantage if their yachts were classed. However, that many unclassed yachts are imperfectly constructed there is not the smallest doubt, and there will be but little difficulty in showing this.

In the succeeding remarks it is not my intention to make any elaborate exposition of the system of yacht construction. There is nothing wonderful about that system, and my object in calling attention to the subject is simply to justify the existence of the "Lloyd's Yacht Register." In the first place, I may remark that the rules for building which have been issued by Lloyd's have been approved by those yacht builders who have hitherto been distinguished for the excellence of their work; in fact, the rules were based upon the practice of these builders, and by making that practice the standard for comparison the faults of construction can readily be traced. Of course it can still be urged that for racing purposes a yacht built up to Lloyd's scantlings would be at a disadvantage if matched against one of lighter scantling; this is undoubtedly true for any given displacement, as the yacht of lighter scantling would carry more ballast, have, as a consequence, greater stability, and would carry a larger area of canvas. But obviously this could be said if Lloyd's had pitched their standard very much lower; and they were not expected to provide for the manufacture of stability for the benefit of racing yachts in framing their rules, but rather to provide rules, by building under which, a yacht owner could be certain of obtaining what, as a prudent man, he required, a strong and durable hull.

But whatever faults the exigencies of competitive sailing may cause builders to commit in the matter of scantling, there is no reason why they should not, even for racing yachts, adopt a proper system of fastening. It is even more important that a racing yacht should be properly fastened, and have great longitudinal strength, than it is that a cruising yacht should be—that is, if she is to be equally durable. Even if the scheme of scantling is reduced

a little below what is required by the rules, the fastenings should not be tampered with; yet, oddly enough, imperfect fastenings is one of the most patent faults in the construction of racing yachts.

I will now briefly glance at some of the more prominent features of yacht building, with a view of showing where defects in construction usually occur. In the first place I will refer to materials. Lloyd's rules provide that a term of years shall be assigned to a class according to the nature of the wood used in the construction, and enjoin that the wood must be of good quality and generally free from defects. The frames or plank can be of teak, oak, pine, elm, greenheart, &c., as the builder pleases, and so can the planking be of any of these woods or of all of them mixed. But almost without exception the frames of yachts are of English oak, and they are usually well squared, with their butts properly shifted, dowelled, or chocked. It is not, however, uncommon to come across a frame in some vessel whose butts come on the same plank; and sometimes the frames of the after body, owing to their not having been canted, are anything but square. Generally frames are free from sap, but sappy timbers are, however, sometimes admitted into a yacht; but as a rule more care is taken in selecting the frame than is generally supposed. Some small yachts are single-timbered, and there appears to be no objection to this plan of framing if the timbers are in whole lengths, and are not grain-cut; occasionally, however, a yacht is built with single timbers in two or more badly-shifted lengths, and are grain-cut, besides, in the way of the bilge. Of course these yachts when under way work badly and leak, and would not be fit to keep the sea for any length of time.

The dead woods are frequently wholly of English elm; and as, owing to the sharpness of yachts fore and aft, the dead woods are deep, and much of them confined, the elm is apt to decay. As enjoined by Lloyd's, the upper portions of the dead woods that have to be housed should be of more durable wood, such as English oak.

For planking, pitch pine is largely used, but there is a serious objection to pitch pine if worked "between wind and water," or in the neighbourhood of the load line; yachts are frequently laid up from year's end to year's end, their sides get dry, and pine is a wood that is very damagingly influenced by alternate wet and dry treatment. According to "Lloyd's Yacht Register," pitch pine is admissible (in the fourteen years' class) if only used to the light water line, with English oak bends and teak topsides, and American elm garboards; but by working all the plank in pitch pine the class would be reduced five years. The best known builders take great care in having proper shifts to the butts of the planking, but frequently a yacht is met with where butts come one above the other in a way that must seriously interfere with the fore and aft strength of the structure.

I have already said that grown wood floors are now seldom used, and an excellent substitute for these are iron floors cast to the pattern of wood floors. Another kind of metal floor has been introduced by Messrs. Harvey & Pryer; it consists of lead cast to pattern, but with angle iron inserted to stiffen the arms of the floors. Either the iron or lead floors are found of the utmost advantage in yacht designing, as they form of themselves an important quantity of ballast, and in the best possible position for influencing the stability of the yacht.

When these iron or lead floors are used a kelson is usually fitted over them in the customary manner, and the fastenings are arranged in exactly the same way as they are in similar-shaped wood floors. During the last twenty years Messrs. Camper & Nicholson have used cast iron floors in vessels of all sizes from 40 to 600 tons, and their experience of such floors, combined with that of other builders who have used them, points to the conclusion that such a form of floor construction is one that can be followed with advantage. The lead floors of Messrs. Harvey and Pryer are equally satisfactory, but are not so likely to be generally used, on account of their costliness.

Another form of floor construction (see Fig. 1) has come much into use of late, and although "floors," as generally understood, are entirely dispensed with, the construction has many advantages, and is wholly unobjectionable if properly planned. A main keel (*d*) is laid down,

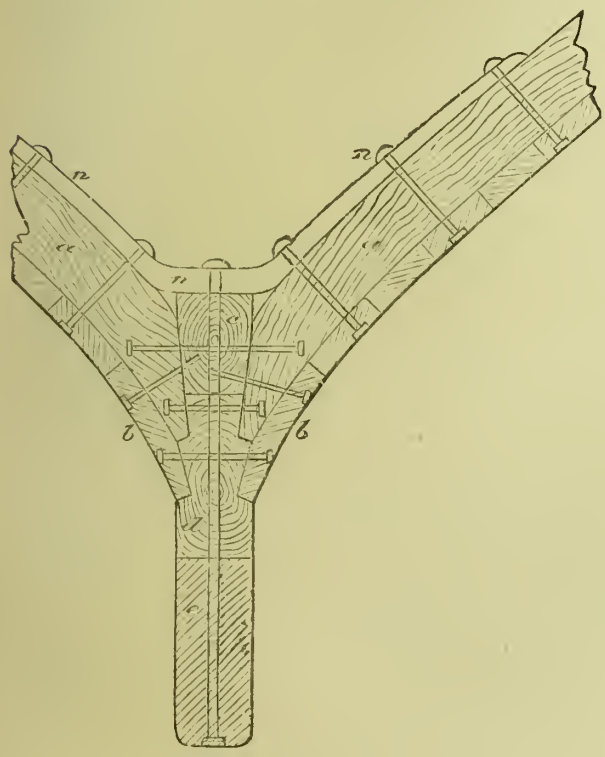


Fig 1.

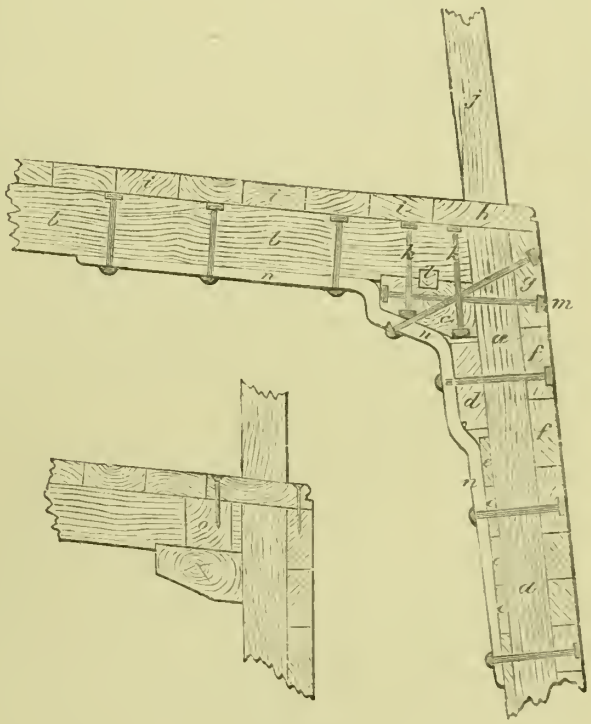


Fig. 2.

and upon this another keel (*c*), called also a kelson, and hogging piece, is worked; the heels of the frames (*a*) are brought down to the sides of these keels, and step-butted into a joggle or into a stepping line. Inside on the backs of the frames a wrought-iron knee-floor (*n*) is fitted, and fastened through the frame and plank (*b*), and through the kelson and keel. Sometimes these iron knees, especially in small yachts, are fitted to the sides of the frames, and if iron fastenings are used in their arms it is best that they should be so fitted, on account of the copper sheathing. Sometimes, however, the fastenings are not driven through the plank, but clinched on a ring on the face of the frames; this plan may secure the iron belts from attack

by the copper, but it is objectionable on account of the difficulty of removing the iron knee-floor without disturbing the plank. It will be found necessary to occasionally remove these knee-floors in consequence of the action of the lead ballast, of which nearly all yachts now carry a portion. A short time since, on examining two yachts which had not had their ballast out for five or six years, it was found that many of the wrought-iron knees were decayed by the continual action of the lead, and there is no doubt that in cases where the lead and iron come in contact under the influence of salt water, they should very frequently be examined. The metal bolts that are used for fastening the iron floors appear to have but little deleterious effect on the iron, and so far as present experience goes are preferable to iron bolts.

I have said that this form of floor construction is an advantageous one if properly planned; but it is not always properly planned, and then nothing can be said in its favour, as it becomes dangerous. A construction to all appearance exactly the same as the other has been resorted to, and indeed the only difference is, that the heels of the frames are not butted into the side of the keel. The fastenings are, generally, exactly similar in arrangement, but often only a couple of iron spikes are used to connect the heel of the timber to the side of the keel. Several large yachts have been constructed on this careless plan, the builder trusting to the strength of the fastenings through the heels of the frames, to the iron knee floors, and to the garboard and the plank to prevent the keel going up through the bottom of the yacht in case she got ashore or was laid ashore.

No harm came of this plan until two years ago, when a vessel weighing about 120 tons ran ashore on some rocks. The garboards were split and crushed out of the rabbets of the keel, and had not the water been smooth there is not much doubt that the keel would have been thrust up through the bottom, as the iron knee floors would not have prevented it. As a proof of the kind of bumping a yacht with a properly constructed floor will stand, I may mention that two years ago a yacht weighing about 150 tons, fitted with the lead floors and kelson of Messrs. Harvey & Pryer previously described, struck on the submerged extremity of the Needles rocks in a strong wind, when she was going about 11½ knots. The sea lifted her and she struck a second time, and the next sea carried her over. The shock was so great that a piece of the outside lead keel as large as one's head was crushed out, and the bolts of the cheeks of the mast were broken; but the hull was in no way injured. There was a slight wrinkling of the copper extending over a radius of 5 feet and 2 feet immediately about the two places where the keel struck, but none of the plank shifted nor was there any leakage. Of course a yacht with such a bad plan of floor construction as I have just described would not be admitted to a class by Lloyd's in the Yacht Register, and I only called attention to the fact that such yachts have been built in order to show the need of rules and surveys whilst building.

The longitudinal strength of yachts has up to within the last two or three years not generally been much considered; and even at the present time yachts of considerable tonnage are built without even an internal bilge strake running along the heads and heels of the timbers. These vessels are necessarily weak, and show their weakness very plainly

by wrinkled copper in the way of the bilge, and by making water in a seaway. Frequently when bilge stringers are worked they are of some unsuitable wood, such as pitch pine, or American elm; and further, these are only imperfectly through fastened, not at all corresponding to the necessary fastenings required by Lloyd's. In fact, one of the most common defects of yacht building is a deficiency of through fastenings, often none being found excepting at the butts. The through fastenings, such as they are, are generally of yellow metal, and the only fault is that their number is so few.

There is no doubt that yacht racing, as before indicated, is largely responsible for weakly constructed vessels; the plank is a little thinner than it ought to be, the top timbers are smaller, the general spacing of the timbers is too great, and the through fastenings are not as frequent as they should be; everything, in fact, consistent with the vessel hanging together without docking for repairs for a year or two, is sacrificed to save a ton or so of weight. So important is this considered for success in competitive sailing that five years ago Mr. John Harvey introduced a most elaborate plan of double skin construction worked over alternate sizes of timbers. By this plan he saved as much as 25 per cent. in the amount of material used in constructing the hull; and as the plan has now been satisfactorily tested in a number of vessels, and has been given a fourteen years' class in the "Yacht Register," it can be assumed that this plan is approved.

When an old yacht comes up to be classed, it is generally found that her frame and plank are sound, and of sufficient siding and moulding, even if the frames are spaced a little too widely; and it is further found that she has been put together with some regard to appropriate shifts of butts; but she generally exhibits a weakness in the matter of through fastenings, and under any unusual strain, such as would result from encountering a gale of wind of a day or two's duration, she would assuredly have made such weakness very patent. In making surveys for the classification of old vessels, Lloyd's surveyors scan the fastenings very closely, and as a rule it is found that yachts built during the last decade, if additionally fastened, are otherwise eligible for a high class.

As a rule the shelf, clamp, beams, and frames are properly fitted (see Fig. 2);* but frequently the through fastenings are insufficient in the shelf and clamp, where it is so necessary that the different parts of the structure should be securely bound together; sometimes, too, in yachts of forty tons and under no shelf, such as could be termed so, is fitted—nothing but a clamp a little thicker than the thickness of the plank. Such constructions are not very creditable to those who plan them, and would not be countenanced by a thorough shipwright. Happily these yachts are few and far between, and the influence of the "Yacht Register" will no doubt cause their disappearance altogether.

Before closing this paper I should like to call attention to two matters contained in the rules in the "Yacht Register." The first of these relate to the length of scarphs allowed in keels. The maximum length of scarph required is 6 feet for a 500 tons vessel.

* *a* is a frame; *b* a beam; *c* the shelf; *d* the clamp; *e* the ceiling; *f* the plank; *g* the sheer strake; *h* the covering board; *i* the deck plank; *j* a bulwark stanchion; *k* through bolts; *l* dowell; *m* through bolt; *n* iron hanging knee; in the small sketch *o* is a filling piece worked between the beams to receive the fastenings of the covering board; the filling piece *o* is bolted to the shelf.

and 3 feet 8 inches for one of 15 tons. For a yacht of 100 tons the scarph must be 4 feet 6 inches. Now, I believe that either of these scarphs are shorter than any of the builders would work to, on account of the great moulded depth of the keels, and I am sure that when a scarph is necessary in the keel of a fifteen tonner, which is not often, that it is not less than 5 feet long. The other matter to which I referred relates to the use of treenails: in the rules contained in the "Yacht Register," it very clearly points out how treenails can be used, and how the using of them will affect the classing of a yacht; in this respect the rules are explicit and clear, as they are in all other respects, but the matter I have some doubt about is the employment of $\frac{7}{8}$-inch treenails in yachts of only 40 tons. The timbers of such a yacht would be 4 inches squared, and many $\frac{7}{8}$-inch treenails through such a frame would very considerably weaken it; and further, the size of treenail is disproportionate to the size set down for a yacht of 100 tons, which has a frame 6 inches squared, and 1 inch treenail. I am inclined to think that the use of tree-nails should cease at 60 tons, and a 5 inch frame; and I believe that as a rule they are not used by builders in yachts whose frames are smaller than 5 inch sided.

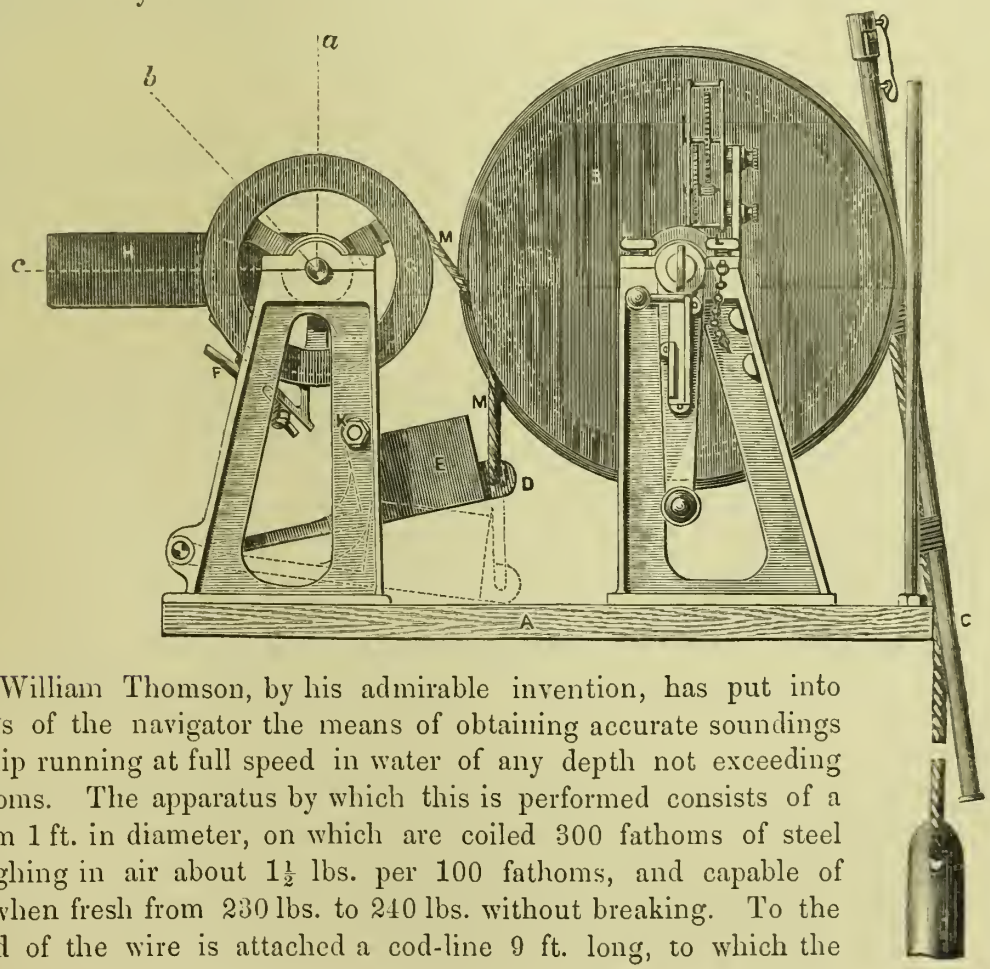
I think it will be agreed that there existed a sufficient need to justify the proposal to establish a society for the supervision of yacht building; and if that society was not established it was only because Lloyd's came forward with a still better scheme, ready prepared and in immediate working order; and further, Lloyd's committee, from their large experience of composite and wood vessels, were able to supplement the rules with appropriate schemes for composite and iron yachts. In this respect "Lloyd's Yacht Register" will be found of most important service, as, whilst many composite and iron yachts were built of undoubted strength and durability, quite as many were weakly built and of very inferior material.

ON SIR W. THOMSON'S NAVIGATIONAL SOUNDING MACHINE.

By P. M. Swan, Esq., Member.

[Read at the Twentieth Session of the Institution of Naval Architects, 5th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

The utility of soundings in practical navigation cannot be denied; yet how often do navigators let hours go by without obtaining soundings, either because of the delay occasioned by the imperfect means at their command, or on account of the danger of rounding-to in heavy weather.



Sir William Thomson, by his admirable invention, has put into the hands of the navigator the means of obtaining accurate soundings from a ship running at full speed in water of any depth not exceeding 100 fathoms. The apparatus by which this is performed consists of a light drum 1 ft. in diameter, on which are coiled 300 fathoms of steel wire, weighing in air about $1\frac{1}{2}$ lbs. per 100 fathoms, and capable of bearing when fresh from 230 lbs. to 240 lbs. without breaking. To the lower end of the wire is attached a cod-line 9 ft. long, to which the

sinker is made fast. A brass tube, a little over 2 ft. long, open at its lower end, and having a moveable cap at its upper end, is lashed to the cod-line. A glass tube, 2 ft. long and $\frac{1}{12}$th of an inch in diameter, coated inside with chromate of silver, and closed at one end and open at the other, is placed with its open end down within the brass tube. A cord wound round the circumference of the drum is so arranged as to form a self-acting brake, which when the sinker is hanging over the stern offers enough of resistance to prevent it from running down, but when it is being hauled in offers very little resistance to the turning of the drum.

When a sounding is to be taken, the brake is released by the hand, so as to leave a force of about 7 lbs. pulling on the cord, by which a resistance of about 5 lbs. is opposed to the wire running out. When the sinker reaches the bottom the drum suddenly stops. The brake is then applied to prevent any more wire running out. The handles, which while the sinker was descending allowed the drum to revolve without being carried with it, are now shipped. The counter is read, and the process of hauling in is commenced.

When the glass tube is brought on board again, it is found that the chromate of silver has been decomposed by the action of the sea-water as far up as the water within the tube has been forced. This shows on a properly graduated scale the depth to which the tube has descended.

To prevent any error arising from the difference of the temperature of the air and the sea into which the glass tube is immersed, the case in which the tubes are kept has double sides, so that the space between may be filled up with sea-water, thus allowing them to be kept at the same temperature as the sea.

In using this machine for sounding there are two things to be observed :—

1. Not to let the wire kink.
2. Not to let the wire rust.

The proper use of the brake prevents kinking, and a tub with lime-water prevents rusting. To show how durable the wire is, I may mention that in one mail steamer the same piece of wire has been in use for nearly two years without a single sinker having been lost. This apparatus is now largely used in the merchant service.

Referring to it, Captain Thomson, of the White Star steamer *Britannic*, says: "I have repeatedly sounded while crossing the 'Banks,' going at the rate of 16 miles per hour, two hands being only required to heave in the line."

Captain Emdelle, of the Transatlantique steamer *La France*, says: "I have been able to make, during a violent gale of wind, exact soundings, easily obtained, and at a time when the use of any other methods would not have given me results in which I could have placed much trust."

Captain Leighton, late of the steamer *Palm*, who first brought this sounding machine into practical use, says: "During the voyage in the *Palm* steamship, I took frequent opportunities of testing the sounding machine when I had a chance of cross bearings, to verify the depths as shown by chart, and always found it most accurate. For instance, going up the Archipelago and just after clearing the Zea Channel, I got a good position by

bearings, chart showing 79 and 76 fathoms; two casts of your glass gave 78 and 75 fathoms. In the Bosphorus also it gave capital results in 30 to 40 fathoms water. The first real use I made of the machine was in the Black Sea during a fog which obscured everything. Wishing to make sure of my position, I put the ship's head for the land and kept the machine at work. After running into 30 fathoms at full speed I slowed down and went in to 12 fathoms, then hauled out to a convenient depth and put her on the course up the coast. When it became clear I found myself in a proper position, and no time had been lost by stopping to sound."

The following letter appeared in one of the Glasgow daily papers of date November 29th, 1877:—

"Sir,—Having noticed about two months ago a letter in your paper containing some disparaging remarks on Sir William Thomson's sounding machine, will you allow me space to give a sailor's experience of it. On the 22nd instant, while scudding before a heavy westerly gale in a mineral-loaded steamer about forty miles S.W. of Scilly, I obtained accurate soundings at noon with Sir William's machine, which indicated 72 fathoms, fine sand, the ship going about 8½ knots. This I verified by sighting Scilly at 5 p.m. The soundings were taken by myself, with the assistance of the steward and carpenter, the sinker and tube being hove in by the two last-named without trouble, although the spray was flying heavily over the taffrail. Now, Sir, I venture to say, with no other apparatus could the soundings have been obtained with so few hands, and so little risk of being washed away from the winding gear. I have had the sounding machine on board for the last six months, but have refrained from giving an opinion till I had tested it, as on this occasion, in exceedingly bad weather.—I am, &c. A Mariner for Forty Years."

On the chart of the English Channel on the wall is shown the track of the *Lalla Rookh*, a schooner yacht of 126 tons, when running before a S.W. gale in thick weather. Referring to it Sir W. Thomson writes:—

"About 5 a.m. on the 6th (August, 1877), I took two casts, and found 98 fathoms (sand and red spots), and 101 fathoms (sand and small shells). The mean, with a correction of 2½ fathoms to reduce to low water, according to the state of the tide at Ushant at the time, was 97 fathoms. Thenceforward I took a sounding every hour till eight in the evening. By writing these soundings on the edge of a piece of paper at distances equal, according to the scale of the chart, to the distances run in the intervals, with the edge of the paper always parallel to the course, according to the method of Sir James Anderson and Captain Moriarty, I had fixed accurately the line along which the vessel had sailed, and the point of it which had been reached with only a verification by a noon latitude. At six o'clock next morning by the soundings and course, with a proper allowance for the flood tide, I must have been about 13 miles magnetic south of the Start, but nothing of the land was to be seen through the haze and rain; and with the assistance of about 10 more casts of the lead (by which I was saved from passing south of St. Catherine's) I made the Needles Lighthouse right ahead at a distance of about 3 miles at 2 p.m., having just had a glimpse of the high

cliffs east of Portland, but no other sight of land since leaving Madeira and Porto Santo. In the course of the 280 miles from the point where I struck the 100 fathom line, to the Needles, I took about 30 casts in depths of from 100 fathoms to 19 fathoms without once rounding to or reducing speed; during some of the casts the speed was 10 knots, and the average rate of the last 220 miles was a little over 9 knots."

The other chart shows the track of the S.S. *British Empire*, while on a voyage from Liverpool to Philadelphia; unfortunately it is not the original chart, as that has been returned to Captain Lecky, who commanded the vessel, and who made the very complete observations you see on the wall, from which the track has been laid down on the chart.

In writing to Sir William Thomson, Captain Lecky says :—

"Until this voyage, I had no good opportunity of putting the sounding machine thoroughly through its facings, but approaching the American coast during foggy weather gave me this time the chance I was seeking. Coming down inside Sable Island, we carried a continuous line of soundings 465 miles, without once slowing the vessel for this purpose. In all we took 27 casts, or an average of one every 17 miles. On the chart which accompanies this letter, you will find plotted the position of the ship at every cast, and I can only say that after the above long run without sights, we made Absecom Light some seven minutes before the hour we stated to the passengers we should see it. It must be admitted that you have succeeded in reducing to the simplest possible operation what used to be a very serious and troublesome job. I think our passengers took as much pleasure in the working of the sounding machine as we did, for in the daytime they would crowd round to witness the casts being taken, and great was their astonishment to see the whole thing done by two sailors and a quarter-master, with an officer standing by."

Before concluding this paper I would point out, that one sounding by itself is of comparatively little use in practical navigation; a series of soundings is almost always necessary to determine a vessel's position. With the ordinary sounding apparatus, to take a series of soundings such as those taken by Captain Lecky would involve an actual loss of time of about $4\frac{1}{2}$ hours, allowing the very moderate estimate of 10 minutes for each sounding; this becomes a serious matter in a run of 30 hours. There is, besides, the chance of the vessel drifting out of her position while the sounding is being taken.

[TABLE.

TABLE OF SOUNDINGS.—S.S. "BRITISH EMPIRE," CAPTAIN LECKY.

Date.	Hour.	Counter	Scale Reading.	Barometer, Air and Water.	Correction.	Actual Depth.	Nature of the Bottom.	Latitude.	Longitude.	Patent Log.	Wind and Weather.
	P.M.	fms.	fms.		fms.	fms.				miles.	
☉ Nov. 17, 1878	6.7	118	50	30″·25 41°–48°	2	52	G. S.	41° 35	66° 23′	—	N.E., Light.
,, ,,	7	105	46	30″·24 48°–49°	2	48	ditto	41° 29½′	66° 38′	—	ditto.
,, ,,	8	105	45	30″·25 48°–50°	2	47	Bkn. Sh. G. S.	41° 24′	66° 52¼′	106½	N.E., Moderate.
,, ,,	10	70	33	30″·25 50°–52°	1½	34½	G. S.	41° 12′	67° 24¼′	—	E., Light.
,, ,,	Midnight	67	31¼	30″·21 50°–52°	1¼	32½	F. G. S.	40° 59½′	67° 56′	160	E.S.E., Moderate.
☽ Nov. 18, 1878	A.M. 2	57	28	30″·15 52°–54°	1	29½	ditto	40° 50′	68° 27½′	—	S.E., Moderate.
,, ,,	4	78	31	30″·09 52°–54°	1	32	F. G. S. & Red Spks	40° 40′	68° 59′	211	E.S.E., Moderate.
,, ,,	6	56	27	30″·02 55°–52°	1	28	G. S.	40° 30½′	69° 32′	—	ditto.
,, ,,	8	76	32	29″·99 56°–54°	1	33	R.	40° 21½′	70° 4′	264	ditto.
,, ,,	10	102	46	29″·90 57°–55°	1½	47½	G. S.	40° 12¾′	70° 37′	—	E.S.E., Fresh.
,, ,,	11.30	155	63¼	29″·85 56°–54°	1½	64¾	Light Grn. M.	40° 6′	71° 1′	—	ditto.
,, ,,	Noon	153	65	29″·82 56°–54°	1½	66½	G. S.	40° 4¼′	71° 8½′	—	E.S.E., Light.
,, ,,	Noon to dy.'s time	152	66	29″·80 56°–54°	1½	67½	Light Grn. M.	40° 2¼′	71° 16¼′	317	Calm, thick Fog.
,, ,,	P.M. 0.40	12[illegible]	61	29″·80 56°–54°	1½	62½	Light Grn. O.	40° 0½′	71° 23¾′	—	ditto.
,, ,,	1.40	100	46	29″·80 55°–55°	1½	47½	F. G. S.	39° 56′	71° 36½′	—	S.W., Light.
,, ,,	4	98	50	29″·75 55°–55°	1¼	51¼	G. S. and ooze with Gn. Spks.	39° 48¼′	72° 2¼′	48	W. by N., Light.
,, ,,	4.15	96	50	29″·75	1¼	51¼	G. S. & F. Gr.	39° 47′	72° 5′	—	W.N.W., Light.
,, ,,	4.25	97	49½	ditto	1¼	51	ditto	39° 46½′	72° 7½′	—	ditto Moderate.
,, ,,	4.33	97	49	ditto	1¼	50¼	F. brown S.	39° 45½′	72° 10′	—	ditto Light.
,, ,,	4.43	105	47	ditto	1¼	48	Do. & Blk Spks	39° 44¾′	72° 12¼′	—	ditto ditto.
,, ,,	4.52	115	48½	ditto	1¼	50	Dk. Brn. S. and Brkn. Sh.	39° 44′	72° 14¾′	—	ditto ditto.
,, ,,	6	77	37½	29″·75 52°–56°	1	38½	Yellow Sand	39° 39¼′	72° 29½′	—	ditto Fresh.
,, ,,	8	66	37	29″·85 50°–54°	1	38	ditto	39° 33′	72° 51′	95	ditto & Clear.
,, ,,	10	40	23½	29″·85 50°–54°	½	24	Sd. with Blk. Spks	39° 25′	73° 18½′	—	ditto ditto.
,, ,,	11	48	28	29″·85 51°–55°	½	28½	Not examined	39° 21′	73° 31¾′	—	ditto ditto.
,, ,,	Midnight	37	20	29″·87 52°–56°	½	20½	Not armed	39° 17¼′	73° 45′	143½	ditto ditto.
☾ Nov. 19, 1878	A.M. 1	—	17½	29″·85 51°–56°	½	18	ditto	39° 13′	73° 58′	153	ditto ditto.

DISCUSSION.

Mr. C. W. Merrifield, F.R.S.: My Lord, it has been the practice of this Institution for some years to encourage bringing forward, and it has generally devoted part of Saturday to that purpose, inventions not bearing directly on the principles of naval construction, or marine engineering, but inventions and devices which are considered to be of general interest to shipbuilders and shipowners. With that view I asked Mr. Swan to be kind enough to explain what I thought would be a matter of great interest to many persons connected with the Institution—Sir William Thomson's sounding apparatus, and at the same time to give us a few of the actual useful results of it, which would be of advantage to us. I hope I shall be expressing the feelings of the meeting generally in tendering my thanks for the kind way in which he has brought it forward, and for the useful information he has given us.

The President: No doubt the meeting generally will concur in the feeling which has just been expressed by Mr. Merrifield.

Admiral Selwyn: My Lord, on behalf of the Navy I desire to say how much we value the additional power thus given to us. Dr. Siemens had given us previously an instrument belonging to the highest class of inventive genius which gave us the deep-sea soundings, that is to say, soundings at the greatest depths with quite sufficient accuracy for all purposes, without a sounding line, but he did not profess to give us anything nearer than an error of one-tenth of the actual depth; but now Sir William Thomson steps forward and gives to seamen the other desideratum, which is, to be enabled at whatever speed the vessel is running, to take soundings accurately, and to do this when the vessel cannot stop—because the competition in speed, having regard to the profits, does not permit them to do so, although they may know that they are running into dangerous positions, because if they were to stop, the profits of their voyage, and the results to the companies they represent, would be very much imperilled. Any instrument, therefore, which enables us to get soundings at all times without stopping the ship, is one which ought to possess not only great interest to the public who travel by sea, but also, I think, is one of very great interest to those shipbuilders who feel like parents generally towards the structures they have originated, and who are very glad indeed to be able to point to a ship which has run for many years without an accident. We, on the other hand, look at it as a question of saving life, and enabling us to pursue our voyage under comparatively safe conditions. The soundings in running across the Atlantic are particularly liable to this difficulty—or rather they have been so hitherto—and that affects one of the greatest, if not the greatest, passenger carrying trades. Now, ships may pursue their way with perfect confidence in the heaviest fogs, as far as shoals are concerned, and it can only be by the gross neglect and incompetence of the persons in charge of the ship, if she ever runs on a reef without their knowing that they are approaching land.

Mr. Henry Liggins: My Lord, many ships within my recollection, and many lives within my knowledge, would have been saved, if this valuable apparatus had been placed on board. In sailing ships, particularly in small ones, there is a great deal of difficulty in heaving to in bad weather to take soundings, and it is a matter which is avoided by captains, because they do not like to give harassing work to their crews. This is a wise invention as well as a practical one, and of the highest value to shipping generally, and the merchant service in particular.

The President: I believe now there is one more paper remaining, but I doubt whether we shall have anything of much more real practical interest than this invention to enable ships to take soundings while in rapid motion, which certainly does appear to me to be a valuable addition to our knowledge and experience in these matters.

ON THE OSCILLOMETER: AN INSTRUMENT FOR MEASURING THE ANGLE THROUGH WHICH A SHIP ROLLS OR PITCHES AT SEA.

By R. Clarke, Esq.

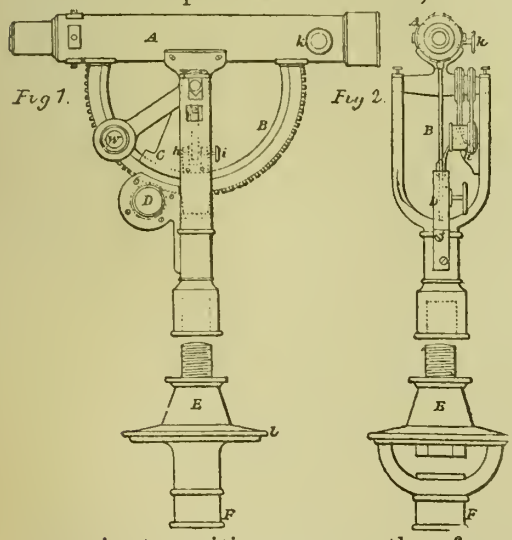
[Read at the Twentieth Session of the Institution of Naval Architects, 5th April, 1879, the Right Hon. Lord Hampton, G.C.B., D.C.L., President, in the Chair.]

It is an ascertained fact that for the purpose under notice, instruments depending upon the principles of gravity and inertia are liable to considerable error.

The most approved method hitherto in use is that of fixing a batten to the ship's side, and noting its deflection below a line drawn from the observer's eye to the horizon (see *Engineering*, December 14th, 1877), a method which the instrument now brought forward is intended to supplant, as it will not only be more convenient to use, but the observations will be more correct.

Description of the Instrument.

In the Figure A, is a telescope with cross webs fixed in its optical centre, similar to the telescope of a theodolite; B is a vertical limb, graduated, and reading right and left to a distance of 45° on either side of the zero point and subdivided to 30′; C is a vernier, graduated to read to 30″. The outer edge of the vertical limb B is toothed and geared into a pinion, D, the latter being made to revolve by a milled head on the end of its axle; an arrangement which gives steadiness of motion to the telescope. E is a socket enclosing a vertical spindle upon which the instrument revolves horizontally; and F is a socket for securing the instrument on to the stand when in use.



The stand may be a pillar of any simple form in which the two requisites of steadiness and stiffness are combined, either fixed permanently or made to screw into a permanently fixed socket placed in any convenient position, upon the fore and aft centre line of the ship; in either case the exposed screw should be covered with a cap.

Adjustment.—Two marks, similar to the zero lines of the limb and vernier, are placed one upon the frame of the instrument, the other upon the vertical limb in such a position that, when brought to coincide, the vertical and optical axes of the instrument shall be at right angles to each other; when these two marks have been so brought to coincide, the arm *g*, which is firmly secured to the arm of the vernier, should be adjusted by means of the capstan-headed screw *h* and the milled-headed screw *i*, until the reading of the vernier to the left of the zero point shall coincide with the angle of dip of the horizon (that adjustment once made the capstan-headed screw *h* should not afterwards be touched). The zeros on the limb and vernier being brought to coincide, and the instrument placed at the proper altitude with its vertical axis perpendicular, the optical axis will be in a line drawn through the centre of the instrument, and produced to the horizon.

Mode of Using.—The instrument being placed upon its stand, the telescope fixed in the proper direction by the clamping screw *l*,* and focused by the milled-headed screw *k*, the line of sight is kept directed to the horizon by turning the pinion D until the roll of the ship attains its maximum, when the angle is read off by the magnifier *m*.

The instrument can be taken apart, and readily packed in a small box, and the following table of "Dip of the Horizon," together with instructions for adjustment and use, should be printed on a card and placed in the box along with it.

Apparent Depression of the Horizon, from Chambers' Tables.

Height of Instrument in feet above sea level. Ft.	Dip in minutes and seconds Mins.	Secs.	Height of Instrument in feet above sea level. Ft.	Dip in minutes and seconds Mins.	Secs.
10	3	3	21	4	26
11	3	12	22	4	32
12	3	21	23	4	39
13	3	29	24	4	45
14	3	37	25	4	51
15	3	45	26	4	56
16	3	52	27	5	2
17	3	59	28	5	8
18	4	6	29	5	13
19	4	13	30	5	18
20	4	20			

* This screw is not shown in the sketch.

CONCLUDING PROCEEDINGS.

Mr. J. D'Aguilar Samuda, M.P.: My Lord, I rise at the termination of the proceedings on behalf of the meeting to express their thanks to you for your kindness in presiding over us. (Applause.) On the present occasion, my Lord, I feel that the members of this Institution have an extra amount of obligation to your Lordship, and at the same time an extra amount of regret in the foreshadowing which your Lordship has made us acquainted with—your desire to bring to a close that most agreeable presidency which you have for so many years exercised over us. But I do hope, in expressing our deep gratitude for all that we have received, that we may yet have the benefit of hearing from your Lordship some words which will encourage us to believe that this is not the last time that we may have that honour conferred upon us, and I am quite sure, knowing the extreme kindness that your Lordship has always shown in listening to everything that is brought to your attention, and the great interest you have always felt in the success of this Institution, that the remonstrance, if I may so call it, at any rate the anxious desire of those members who called upon your Lordship, and begged you to reconsider in some degree that step which you expressed it was your intention to take, may receive at your Lordship's hands that amount of consideration which will be a matter of the deepest interest to ourselves, if it should result in your Lordship continuing for some time longer to preside over us in a manner that we feel we have no chance of being presided over in the future by any successor of your Lordship. (Applause.) I beg to propose a vote of thanks to your Lordship.

Mr. J. Scott Russell, F.R.S.: My Lord, it is my privilege as an elder brother of this family of craftsmen, who have so long enjoyed the pleasure of meeting annually under your presidency, to say that we are all deeply grateful for your having presided over us on this occasion. Our gratitude extends back for the twenty years in which we have as a young Institution prospered under your direction, and benefited and matured by your wisdom. (Applause.) This family of yours, my Lord, is now of the age of twenty, and we hope that you will still continue to be our guardian until we attain the maturity of twenty-one years, at which time we hope by the wisdom you have taught us, and the kindness you have shown us, we may be bound together in a family, and be able to take care of ourselves. We shall do that, I am sure, to the best of our ability, but we shall always remember you as our kind and loving father. (Applause.)

The President: Mr. Samuda, Mr. Scott Russell, and Gentlemen, this is the twentieth occasion, as my friend, Mr. Scott Russell, has just said, on which I have had to acknowledge a very kind vote of this sort at the conclusion of our Annual Session, and I beg now to thank you very cordially for the kind way in which you have passed this vote, and to thank my distinguished friends, Mr. Samuda and Mr. Scott Russell, for the terms in which they have been so kind as to propose it. But, gentlemen, Mr. Samuda and Mr. Scott Russell have both referred to circumstances which I think make it desirable that I should not confine myself now strictly to thanking you for the compliment that you have paid me. Mr. Samuda referred to the fact that in my opening address on Thursday morning, I took occasion to intimate the wish which I have for some time felt, that having presided over this Institution for twenty years, I should be relieved from the duties of the chair, and that the presidency should be turned over to some other and younger member of the Institution. This I thought to be my duty. I thought it due from the respect I feel for this Institution, and the deep interest that I have always entertained for the objects which it has in view, that I should publicly intimate the wish I felt to be relieved from the presidency. It would hardly have been consistent with my feelings to have

done otherwise. But yesterday, gentlemen, I received, quite unexpectedly, a deputation of four very eminent members of this Institution, who came to ask me to reconsider the decision which I had intimated on the day before. They put it to me in the kindest way; they fully recognised the fairness of my desire to be relieved from the chair, but they requested that I would reconsider my determination to the extent of consenting to preside one more year, and, of course, that presidency would include the Annual Meeting of 1880. This request was supported by what I could not deny to be a good reason. It will be in the recollection of all you gentlemen present that part of our proceedings yesterday was to adopt a new code of bye-laws, which, in fact, amount to a reorganisation of this Institution, and to an entirely new mode, and a more popular and more judicious mode I am willing to believe, of electing our various officers and those who conduct our affairs; and my friends who did me the honour to wait upon me yesterday morning pressed upon me that it would be very desirable that my Presidency should be co-extensive with our old system, and that whoever has the high honour, for so I consider it, of succeeding me in this chair should commence the new system. Gentlemen, I cannot deny that this was a very reasonable ground for wishing me to postpone, for at least one year, my resignation of the office of your President, and therefore I feel bound upon this occasion to say I am willing to accede to that request. (Applause.) I hope, provided my health and strength continue good, to once more take the chair next year. I need not say any more now than to assure you of the very deep sense I entertain both of the support I receive from the members of this Institution, and the kind spirit in which that request has been preferred to me on this occasion. I beg once more to tender my sincere thanks.

Mr. C. W. Merrifield, F.R.S.: My Lord, I think I shall only be expressing the feelings of this meeting in saying that we are extremely unwilling to separate without expressing our cordial gratitude to the President and Council of the Society of Arts, and to the Society itself, for the liberal manner in which they have on this, as they have on so many occasions, granted us the use of this beautiful hall for our meeting. It has been of the greatest advantage to our Society in every respect, and I feel sure that the rest of the Institution will join cordially with me in expressing their thanks to the Society of Arts for it.

Mr. John R. Ravenhill: My Lord, I have been requested to second the resolution which Mr. Merrifield has just brought under your notice, and I beg most cordially to do so.

The President: It is with very great pleasure that I put to the meeting this proposal. It is a vote which I think we are bound in gratitude and courtesy to adopt. It is a vote which we have adopted every year, and I am sure you will all join now in cordially thanking the Society of Arts for the liberal spirit in which they have every year during the whole period of the existence of this Institution placed the use of this excellent hall at our disposal, well adapted, as you must all feel it is, to carry on the business of this Society. I need not ask you for a show of hands, as everyone, I am sure, will join in that vote.

Carried by acclamation.

Mr. John Humphrys: My Lord, I have very much pleasure in rising to move a vote of thanks to the Council of this Institution. I think we owe a great debt of gratitude to them for the labour and attention which they must necessarily give to the management of the proceedings of this Institution.

Mr. N. E. Porter: I have very much pleasure in seconding that resolution.

The President: Gentlemen, I suppose I am quite at liberty to put the vote—That the thanks of this meeting be given to the Council for the care and attention they have bestowed on the affairs of this Institution.

Carried unanimously.

MEMOIR OF THE LATE JOHN PENN, F.R.S.,

Vice-President of the Institution of Naval Architects.

THE Institution has to deplore the loss of Mr. John Penn, who was one of its original founders and first Vice-Presidents. Mr. Penn was born at Greenwich in 1805, and received his mechanical education at the workshops of his father, who was a mechanical engineer and millwright. Mr. Penn managed these works for many years, and they passed into his possession on the death of his father in 1843. At an early age he introduced the grasshopper engine, and employed one of them of 6 horse-power to drive the machinery in the workshop at Greenwich. He soon turned his attention to steam navigation, which was then comparatively in its infancy, and designed and built two sets of beam-engines of 40 horse-power each for the steamships *Ipswich* and *Suffolk*, and shortly afterwards, in 1835, four more of the same type for vessels running between London and Greenwich. Though the oscillating engine was not invented by Mr. Penn, he early recognised its great advantages, and applied it with improvements of his own to the river steamer *Endeavour*, which ran between London and Richmond. The improvements which he effected were so great that in 1844 he was employed by the Admiralty to supply new engines of the oscillating type to the Admiralty yacht *Black Eagle*. This task he accomplished with great success. His new engines and boilers occupied only the same space as the old ones, and developed double the power. In consequence these engines came into general use, and Mr. Penn afterwards received orders to engine the Royal yacht *Victoria and Albert* and the *Great Britain* steamship.

His success in improving the screw-engine was perhaps even more marked. One of the great objects to be accomplished in order that screw-propellers might be usefully applied to men-of-war, was to keep the engines well below the water-line in a position of comparative security against the effects of shot and shell. One of the solutions of this problem was the introduction of the well-known type of trunk-engines, in which the intermediate gearing was done away with, and the piston coupled direct to the crank without the intervention of a piston-rod, an arrangement which enabled the engines to be kept low and compact. The first vessels fitted with these engines were the *Arrogant* and *Encounter* in the year 1847, and these proved so successful that the type came into general use for men-of-war, and before Mr. Penn's death, his firm had fitted up no less than 230 vessels with trunk-engines of sizes, varying from 20 horse-power for small gunboats, to 8,832 horse-power, which latter power was developed by the engines of H.M.S. *Neptune*, and is the greatest power ever attained by a single pair of cylinders. Mr. Penn also paid great attention to the subject of increasing the piston speeds of screw-engines, and by careful design and great perfection in workmanship he was enabled to make improvements in this direction, which were attended by large savings in the weights of the machinery and the spaces occupied by it.

Another great service which Mr. Penn rendered to the cause of screw-propulsion was the application of lignum vitæ to the sea-bearings of screw shafts. For a long time the success of the screw as a substitute for paddle-wheels was rendered doubtful by the rapid wear of every description of metal bearing. After a series of experiments had been carried out for him by Mr. Francis Pettit Smith, Mr. Penn patented in 1854 the use of lignum vitæ for this purpose, and applied the invention with the most complete success, in the first instance to H.M. corvette *Malacca*, for, previously, this vessel had been wearing away her outer screw shaft bearings at the rate of $3\frac{1}{2}$ ounces per hour, but after the introduction of wooden bearings she was enabled to steam 15,000 miles, the wear at the end of this voyage being scarcely appreciable.

Mr. Penn rendered great service to his country during the Crimean War by undertaking and actually carrying out the fitting of 121 vessels with engines. Of these 97 were gunboats, fitted each with a 60 horse-power engine. In carrying out this work he proved his talents for organisation by the systematic way in which he availed himself of the assistance of other firms in duplicating the separate parts.

Mr. Penn joined the Institution of Civil Engineers in 1826 as an Associate. He was elected Member in 1845. He became a Member of the Institution of Mechanical Engineers in 1848, and has twice been President of that Institution, viz., in 1858-59 and 1867-68. He was made Fellow of the Royal Society in 1859, and in 1860 he was present at the founding of the Institution of Naval Architects, at which meeting he was also elected Vice-President, and continued as such till his death, which took place at his residence, Lee, in Kent, on the 23rd of September, 1878.

MEMOIR OF THE LATE WILLIAM FROUDE, LL.D., F.R.S.,

Vice-President of the Institution of Naval Architects.

William Froude was born in 1810, and was a son of Archdeacon Froude, of Dartington, Totnes. He was a King's Scholar at Westminster, and went thence to Oriel College, Oxford, where he was the pupil of his brother Hurrell Froude, and of John Henry Newman. He would dwell in after years with gratitude on the sound training in Mathematics which he received from his brother, and which resulted in his obtaining a first class in the Mathematical Class List.

On leaving Oxford, Mr. Froude adopted the profession of a Civil Engineer, and became a pupil of Mr. H. R. Palmer, and in 1838 an assistant of Mr. Brunel, under whom he was engaged on the works of the Bristol and Exeter Railway, and in other engineering work. Mr. Froude's regular professional work ceased in 1846, when he retired on account of his father's failing health; but he continued to occupy himself in scientific investigations of a high order, as for example, the discharge of elastic fluids under pressure, and the resistance experienced by a plane moving obliquely in a fluid. Mr. Froude also gave his assistance in

the solution of various practical questions in the district in which he lived. Among the most noteworthy of these may be mentioned the advice he gave (1863 to 1866) with regard to the Waterworks of Torquay, where, by a carefully-pursued investigation into the causes of the deficient delivery of the water main, and by his skill in carrying out a remedy suggested by the late Mr. Appold, he saved the town for many years the expense of laying a new main at a cost of some £30,000.

His intimate friendship with Mr. Brunel led him back at times to take part in engineering work, and on the occasion of the launch of the *Great Eastern* (1857) Mr. Froude conducted a series of experiments on friction, and on the behaviour of the ship herself.

It was also in reference to the *Great Eastern* that, in 1856, at Mr. Brunel's request, Mr. Froude undertook the inquiry into the rolling of ships, which will always be connected with his name.

Mr. Froude grasped the true elementary nature of the action of wave-water on a floating body, and of the effect of successive waves on a ship exposed to their action. As an essential stepping-stone to progress in the inquiry, he established on thoroughly sound principles the mechanical possibility of that form of motion known as the trochoidal sea-wave, a subject which has been discussed by other writers. The leading principles of his views he exemplified by experiment, and at an early meeting of this Institution (1861) he brought forward the leading propositions with regard to the rolling of ships, which form the groundwork of the now established theory on the subject. The building up of the theoretical structure thus started was a task which he rapidly proceeded with; he appreciated and applied in their correct bearing on the subject the several causes modifying the application of the elementary principles, and he explained his views in papers contributed to this Institution.

Having brought the inquiry to this stage Mr. Froude was fortunate in having an opportunity of investigating the subject experimentally. By still-water rolling experiments on a large number of Her Majesty's ships (1871 to 1876) he obtained by self-recording apparatus a definite measure of the resisting force offered by the water to the rolling motion of a ship in still water. The results when analysed he reduced to mathematical expressions which represented the action of this force. He also measured the rolling at sea of the *Comet* gunboat (1872), and of the *Greyhound* (1872), using for that purpose a simple arrangement of a pointer worked by an observer, combined with a self-recording pendulum.

Mr. Froude, appreciating the effect of "resistance" in checking rolling, had from an early date recommended the extended use of bilge-keels as a remedy. They had been applied with much success to the Indian troop-ships, and as a member of the "Committee on Designs," Mr. Froude had made instructive experiments with large models of the *Devastation* fitted with bilge-keels of various sizes (1871). Further to test the importance of bilge-keels, comparative trials were made off Plymouth (1872) of two ships of the same class, the one (*Greyhound*) with bilge-keels, and the other (*Perseus*) without; the results have been published.

When the *Devastation* made her sea-going trials Mr. Froude accompanied the vessel in trials off Queenstown and off Bantry (1873), and on her first voyage to the Mediterranean (1875). Though seas of the character most suitable for experiment were not found on either occasion, she met with a very heavy sea off Bantry, and with a long and low swell off Lisbon, which supplied valuable experimental data.

In the *Greyhound*, when tried off Plymouth, and subsequently in the *Devastation*, Mr. Froude used an instrument he had devised, in which a heavy delicately-hung fly-wheel which took the place of the pointer, and a suitably placed pendulum, recorded their behaviour on a diagram, and not only measured the ship's oscillation, but also measured, as a differential phenomenon, the mean wave acting upon the ship, with a degree of exactness exceeding that with which it had hitherto been possible to ascertain the profile of the surface wave of the sea.

As a member of the *Inflexible* Committee, Mr. Froude had the opportunity of testing experimentally on a large model the extent to which loose water in a ship may operate to check rolling, a point which he had for some time thought of much importance, and which had been one of the features of the design of that ship. The quelling effect was most marked.

Aided by these experimental researches, Mr. Froude had brought to definite terms the quantitative determination of the leading features in the theory of rolling, and may in fact be considered to have established not only the leading true principles, but to have put these into a workable shape. Not, indeed, that he considered the inquiry exhausted, for at his death he had in preparation at the experiment tank at Torquay means for prosecuting further important branches of the investigations, by means of experiments on models floating in artificially-formed waves. The same apparatus was also intended for the investigation of the cognate subject of pitching, on which Mr. Froude had made observations in the *Devastation* off Bantry, and on which he had made valuable reports to the Admiralty. About six years ago Mr. Froude prepared a memorandum for the guidance of officers of the Royal Navy in making the observations of ocean waves which are ordered to accompany observations of rolling. A mass of valuable data is thus being acquired.

The value of Mr. Froude's labours on the subject of rolling is probably exceeded in a commercial, if not in a scientific, point of view by that of his work in regard to the resistance and propulsion of ships. Being an able yachtsman, and also experienced in steamship trials, the problem of the resistance of the water to a vessel moving through it had been amply brought before his notice; and when he had the opportunity he endeavoured to feel his way towards a consideration of the subject by experiments with models. While thus engaged, he set himself to consider, and succeeded in arriving at, the law of comparison by which a ship's resistance may be deduced from that of her model. He exhibited the general correctness of the proposition by conclusive experiments.

Mr. E. J. Reed, then Chief Constructor of the Navy, saw the importance of encouraging a method of thus economically ascertaining the resistance of ships; and having visited Mr. Froude, and seen the experimental results, he requested Mr. Froude to put into

a definite proposal his willingness to conduct a series of experiments for the Admiralty. Mr. Froude's offer was accepted at a later period, not without some public questioning as to the comparative utility of small-scale and full-sized experiments. An experimental tank with its machinery was erected near Mr. Froude's house, at Chelston Cross, Torquay, and for the last seven years most valuable work has been there carried on. All the recent ships of the Royal Navy have been subjected in model to these experiments. Several kindred experiments were also tried, and a series of experiments on form has been in progress, with a view to obtaining generally applicable principles. Also, as a preliminary to the other work, a series of experiments on surface friction were tried.

In 1871, on the recommendation of the "Committee on Designs," an important experiment was tried under Mr. Froude's superintendence. A full-sized ship, the *Greyhound*, was towed at various speeds and the tow-rope strain accurately measured. The results verified the law of comparison on which the utility of the model experiments chiefly depends.

The problems of resistance naturally lead to those of propulsion, as many of Mr. Froude's papers will testify, and in this subject Mr. Froude has rendered important service by pointing out, measuring, and dividing into its several elements the waste of power connected with the use of propellers, and of the screw propeller especially. The above-mentioned *Greyhound* towing trial, compared with the results of her steam trials, marked sufficiently the magnitude of the waste. One important element of this waste, on which Mr. Froude had made some experiments on a small scale more than twenty years ago, is the prejudicial action of the screw propeller when placed too near the stern of the ship, where it wastes power by diminishing the water pressure at the stern of the vessel. The action of screw propellers formed an important part of the investigations at Torquay, and the amount of the augmentation of resistance caused by the action of the propeller behind various forms of ships has been determined with considerable exactness. The surprising results of the early steam trials of H.M.S *Iris* led Mr. Froude's attention more closely than before to the action of the propeller itself, and his paper on the Elementary Relation between Pitch, Slip, and Propulsive Efficiency, read at the April Session of last year, was the last of his many contributions to our Transactions.

For the investigation of the propulsion of ships, it being of the greatest importance to obtain accurate and detailed records of the phenomena attending their performance under steam, Mr. Froude had for some time past turned his attention to measured mile trials. His paper on Mr. Denny's "progressive speed trials" was one of the fruits of this. A number of elaborate observations were made by Mr. Froude during the recent measured mile trials of the *Iris*, resulting in a large body of interesting information, and his investigations on this subject will probably lead to a great increase in the practical and scientific value of steam trials.

To determine the power wasted in marine engines, and so to estimate the proportion of the whole indicated horse-power actually delivered to the screw propeller, Mr. Froude, at the request of the Admiralty, designed and superintended the construction of a dynamometer to be capable of measuring 2,000 horse-power. This is now nearly ready for

trial. He explained the principle of the instrument to the meeting of the Mechanical Engineers at Bristol in 1877.

Mr. Froude, in addition to his papers that have appeared in these Transactions, made several important contributions to naval science, and some of his reports to the Admiralty have been published in Blue Books and in the Reports of the British Association.

He also, though rarely, gave popular explanations of the subjects he was dealing with. In his opening address as President of the Mechanical Section of the British Association at Bristol in 1875, he gave an intelligible exposition of the doctrine of stream lines as far as it affects the fundamental principles of the resistance of ships. This address he afterwards delivered as a lecture at the Royal Institution. To the Science Teachers assembled at South Kensington in 1876 he delivered a lecture on Propulsion, remarkable for the clearness with which the leading features of the subject were classified and described.

Mr. Froude was an able workman; with his own hands he executed many most accurate pieces of work, and was thus apt at truly testing the workmanship of others. His mechanical skill is shown by the design and details of the many beautiful machines he contrived for his various investigations. The model cutting machinery; the happy idea, the result of thoughtful consideration, of using hard paraffin for the models; the highly sensitive governors used in the various instruments; the resistance-recording apparatus for the model experiments; the large machine for measuring the tow-rope strain in the full-sized ship trials; the rolling recording instrument; and, lastly, the large screw-engine dynamometer,—all bear witness to his mechanical skill. The principle of the last-mentioned machine especially was the legitimate outcome of a correctly followed search for a means to the desired end.

The importance of his work was appreciated by his brother scientific men. He was elected a Fellow of the Royal Society in 1870. In 1876 he received the honorary degree of LL.D. at the University of Glasgow, and in the same year he received the Royal Medal of the Royal Society "for his researches both theoretical and experimental on the behaviour of ships, their oscillation, their resistance, and their propulsion." In presenting the medal to Mr. Froude, the President of the Royal Society said: . . . "No one indeed has ever done more, either theoretically or practically, for the accurate determination of a ship's motion, whether in propulsion or in waves, than Mr. Froude. Without undervaluing other modern writers, it is not too much to say that his investigations at present take completely the lead in this very important question—most important to a maritime nation."

His services were highly appreciated by the Lords of the Admiralty, and by the constructive staff of that department, and more to him than their cordially expressed gratitude, was the knowledge that in every sense of the word he enjoyed their confidence.

Mr. Froude went out to the Cape in H.M.S. *Boadicea*, in December, 1878, for a holiday, and was on the point of starting for England, refreshed in mind and body, when he was taken ill with dysentery. He died on May 4th. He was followed to his grave in the Naval Cemetery, Simons Town, by men and officers of Her Majesty's ships, as was fitting for one who had rendered such great services to the country in relation to the Royal Navy. His death is a heavy blow to science, and more especially to that branch of science with which our

Institution is concerned. The number and value of his contributions to our Transactions will show the greatness of our loss. The modesty and charm of manner which characterised his utterances when taking part in our debates will be long remembered by those who heard him.

It is understood that in the official letter of condolence sent by the Lords Commissioners of the Admiralty to Mr. Froude's family, they expressed strongly their sense of the services rendered "to the Navy and to the Country, in making his great abilities, knowledge, and powers of "observation available for the improvement of the designs of ships, without reward or any "other acknowledgment than the grateful thanks of successive Boards of Admiralty." The value of his services to naval architecture has been as fully recognised abroad, for it was in the interests of a general progress in the knowledge of the laws of Nature that he laboured.

NOMINAL INDEX.

BEING AN ALPHABETICAL LIST OF THE NAMES OF THE AUTHORS OF PAPERS READ BEFORE THE INSTITUTION, AND OF THE SPEAKERS WHO TOOK PART IN THE DISCUSSIONS WHICH FOLLOWED THE READING OF THE PAPERS.

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Anonymous	Memoir of the late William Froude	263
	Memoir of the late John Penn	264
Barnaby, Nathaniel, C.B. ...	*Armour for ships*	27
	Weight of Millwall shield	33
	Correct dimensions of the *Devastation* armour	34
	Reason for planing projectile to suit Whitworth's gun ...	40
	Reply to discussion on his Paper	45
	The treatment of steel for shipbuilding	147
	The cost of men-of-war	148
	The effect of black oxide on steel plates immersed in salt water	225
	Criticism on Admiral Selwyn's proposed ironclad ...	225
Brown, Captain	On chilled projectiles	40
Clark, Latimer	*On the double power floating dock and the hydraulic grid* ...	204
	Reply to discussion on his Paper	210
Clark, R.	*The oscillometer: an instrument for measuring the angle through which a ship rolls or pitches at sea*	259
Colomb, Captain	On the necessity of keeping the surface of screws smooth	131
Cotterrill, James H.... ...	*On the minimum area of blade in a screw propeller necessary to form a complete column*	152
Denny, William	Mr. Maginnis' Paper on screw propellers	100
	Mr. Griffith's experiments	110
	Trial trip experiments	127
	Initial friction of marine engines	128
	Trials of the *Merkara*	142
	The use of steel for shipbuilding with reference to Lloyd's rules	143
	H.M.S. *Hecla*	143
	Mechanical shipbuilding	235
	Proper quality of iron of shipbuilding	238
Dicey, Captain	The comfort of twin ships in rough weather	176
Edwards, H....	Proposed method of strengthening ships	240
Flannery, J. F.	Mr. Maginnis' paper on screw propellers	102
	Strains of crank shafts	193

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
Gray, J. Macfarlane... ...	New method of strengthening tubes of guns	52
	The thrust of screw propellers	100
	Necessity of having copies of Papers circulated to members	160
	The adjustment of crank shafts	192
	New method of using the indicator diagram	192
Griffiths, R.	*On the resistance given to screw ships by the action of the screw propeller, and how to remedy it*	104
	Reply to discussion on his Paper	113
Hampton, the Rt. Hon. Lord	See President, The.	
Harland, Edward James ...	Trial trip experiments	111
	Cost of men-of-war	148
	Length of trial trips	148
	Details of H.M.S. *Hecla*	148
	Education of Naval Architects	149
	Experiments on skin friction	172
	Improvement of the Channel service	173
Henwood, Charles F. ...	Admiral Robinson's Paper on armour for ships	33
	On Hughes' Stringers	33
Humphrys, James	*On a system of mechanical shipbuilding*	232
	Reply to discussion on his Paper	242
	Proposing vote of thanks to Council	262
John, William	The action of Lloyd's Committee with regard to steel ...	144
	The details of H.M.S. *Iris*	144
	The use of the double bottom	145
	The connections between the hulls of the *Calais-Douvres* and the *Castalia*	169
	The proper quality of iron for shipbuilding	238
	The action of Lloyd's Committee with regard to steel for shipbuilding	238
Kemp, Dixon	Differences of opinion between Mr. Scott Russell and Mr. Colin Archer	85
	On Lloyd's yacht register	244
Kennedy, Prof. Alex. B. W...	The nature of experiments on the screw propeller ...	109
Kirk, A. C.	Trial trip experiments	111
	Initial friction of marine engines	129
	The effect of currents on twin screw propellers	130
	The strength of crank shafts	191
Liggins, Henry	The history of twin ships	170
	Calais-Douvres in rough weather	171
	The steering power of the *Calais-Douvres*	171
	The value of deep sea sounding machines	258
Mackie, S. J....	On the relative advantages of Mr. Hughes' backing ...	34
	Explanation of the weight of the Milwall shield... ...	38
Mackenzie, H. S.	Untrustworthiness of trial trips	112

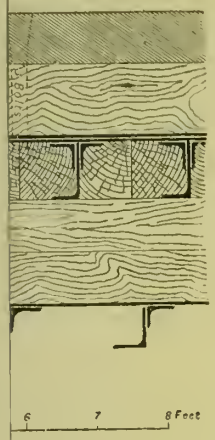
Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
	Mr. Griffith's experiments with screws	113
Mackrow, George C... ...	*On twin-ship propulsion*	162
	Reply to discussion on his Paper	178
Maginnis, Arthur J.... ...	*A few years' experience of the screw propeller*...	88
	Reply to discussion on his Paper	102
	Crank-shafts on transatlantic steamers	194
Martell, Benjamin	Asks question concerning trunks for screw propellers ...	108
McMillan, J., Jun.	Mechanical rivetting...	241
Merrifield, C. W., F.R.S. ...	*On naval guns*...	46
	Reply to discussion on his Paper	58
	Mr. Scott Russell's Paper on the true nature of the wave of translation	84
	Experiments on models and full-size ships	110
	Mr. N. E. Porter's paper on the legal load-line	203
	Value of floating docks	210
	Security of butt-joints in tension in ships built on Mr. Humphry's plan	241
	Mr. P. M. Swan's Paper on Sir W. Thompson's navigational sounding machine	258
	Proposing vote of thanks to the Society of Arts	262
Milton, J. G....	*On some of the causes of failures of crank-shafts in marine engines*	180
	Reply to discussion on his Paper	195
Parker, William	Trial trip experiments	112
	Consumption of coal of the engines of the *Calais-Douvres*...	173
	Mr. Leslie's experiments before building the *Calais-Douvres*	174
	Speed and horse-power of the *Calais-Douvres*	174
	Commercial success of the *Calais-Douvres*	174
Porter, N. E....	*On the legal load-line, and some remarks relating to the principles of safe freeboard*	196
	Seconding vote of thanks to the Council	262
President, The	*Opening address*	xxii.
	Disadvantages of discussing two papers together... ...	45
	Mr. Merrifield's Paper on naval guns	58
	Mr. Scott Russell's Paper on the true nature of the wave of translation	86
	Mr. Wright's Paper on the steam trials of H.M.S. *Iris* ...	131
	Work done at the meetings of the Institution	150
	Necessity of having copies of papers circulated to members	161
	Mr. G. C. Mackrow's Paper on twin-ship propulsion ...	179
	Point of order	239
	Mr. P. M. Swan's Paper on Sir W. Thomson's navigational sounding machine	258
	Returning thanks for vote of thanks	261
	Concerning his resignation of Presidency	262

Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
	Putting votes of thanks to the meeting	262
Ravenhill, John R.	Former difficulties in the manufacture of wrought-iron guns	57
	Trial-trip experiments	111
	Mr. Wright's Paper on the steam trials of H.M.S. *Iris* ...	130
	Coal consumption of the engines of the *Calais-Douvres* ...	175
	Possibility of improving Channel ships	175
	Crank-shafts and reasons why they fail	194
	Consumption of coal in marine engines	224
	Perkin's system of boiler	224
	The boilers of the *Filga*	228
	Seconding vote of thanks to the Society of Arts	262
Robinson, Admiral Sir R. Spencer, K.C.B., F.R.S.	*On armour plating ships of war*	3
	Reply to discussion on his Paper	42
	Point of order	45
	Advocates breech-loading	50
	Bursting of *Thunderer's* gun...	51
Russell, John Scott, F.R.S...	Approves of Hughes' backing principle	36
	Modification necessary to introduce into the *Warrior* class of vessels, to render them efficient against modern ordnance	37
	Relative advantages of steel and iron armour	37
	Faults of Woolwich guns	56
	Dangers of hydraulic loading for guns	56
	On the true nature of the wave of translation, and the part it plays in removing the water out of the way of a ship with the least resistance	59
	Necessity of having copies of papers circulated to members	161
	Mr. Latimer Clark's Paper on double-power floating-dock	209
	Criticism on Elder's and Popoff's vessels	227
	Holyhead mail steamers	228
	Point of order	239
	Mechanical shipbuilding	240
	Seconding vote of thanks	261
Ryder, Admiral	On the surface friction of screws	131
	Skin friction of bows of ships	131
Samuda, J. D'Aguilar ...	Apparent discrepancy between Sir Spencer Robinson and Mr. S. J. Mackie	38
	Disapproves of Mr. Hughes' system	39
	The advantges of steel armour	39
	The danger of bolt holes in armour plates...	40
	The action of Lloyd's with reference to steel for ship-building	145
	The use of double bottoms for ships of war	147
	The speed of the *Mahroussee*...	147

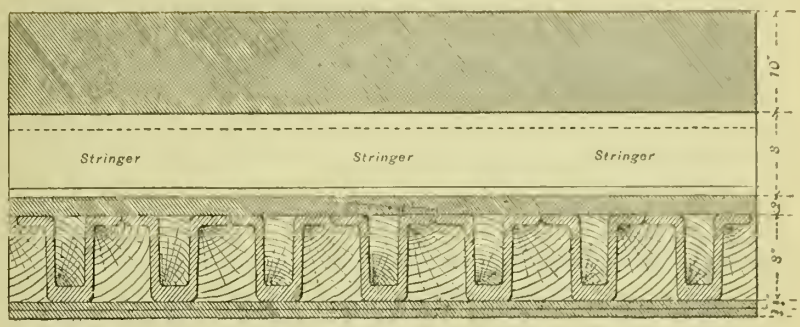
Name of Writer or Speaker.	*Title of Paper*, or Subject under Discussion.	Page.
	Criticism of Admiral Selwyn's ironclad	226
	Mr. Humphry's method of shipbuilding	237
	Proper quality of iron for shipbuilding	237
	Point of order	239
	Proposing vote of thanks to the President...	261
Scott, Captain	Working guns and turrets	52
	Loading heavy guns	53
	The improvements which ought to be effected in the Woolwich guns	54
	Bursting of the Woolwich gun	54
Scott, Michael	Powder required to rotate shot	57
	Cost of Whitworth's steel for guns	57
Selwyn, Admiral	The mode of resistance of armour plates	41
	Steel for guns	55
	Disadvantages of slow burning powder for breechloaders...	55
	The value of floating docks	209
	On the most powerful ironclad	211
	Reply to discussion on his Paper	228
	Deep-sea sounding machines	258
Swan, P. M.	*On Sir William Thompson's navigational sounding machine* ...	253
Thornycroft, J. I.	Speed of water through screw disc	108
	Steering of screw vessels	108
	Coal carrying power of torpedo boats	142
	Mr. White's Paper on H.M.S. *Iris*	142
West, Henry H.	Strains on crank shafts	192
White, William Henry ...	*The structural arrangements and proportions of H.M.S. "Iris"*	133
	Reply to discussion on his Paper	150
	Mr. Cotterill's Paper on the minimum area, &c.	160
	Mr. Humphry's Paper on mechanical shipbuilding ...	239
Wimshurst, James	Loss of power in screw propellers	129
Wright, James	*The steam trials of H.M.S. "Iris"*	117
	Reply to discussion on his Paper	131
	Necessity of having copies of papers circulated to members...	160

UNWIN BROTHERS, PRINTERS, LONDON AND CHILWORTH.

Fig. 6

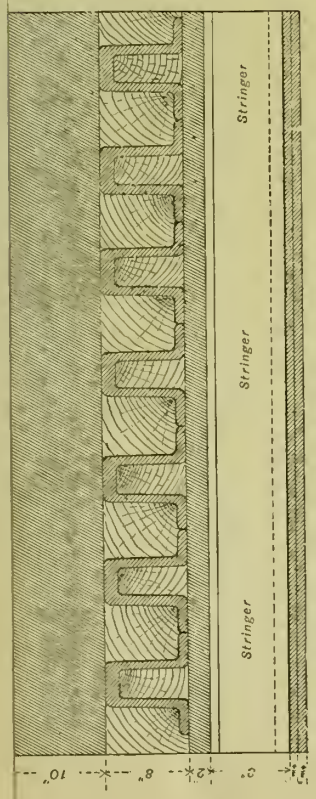


HORIZONTAL SECTION (7).

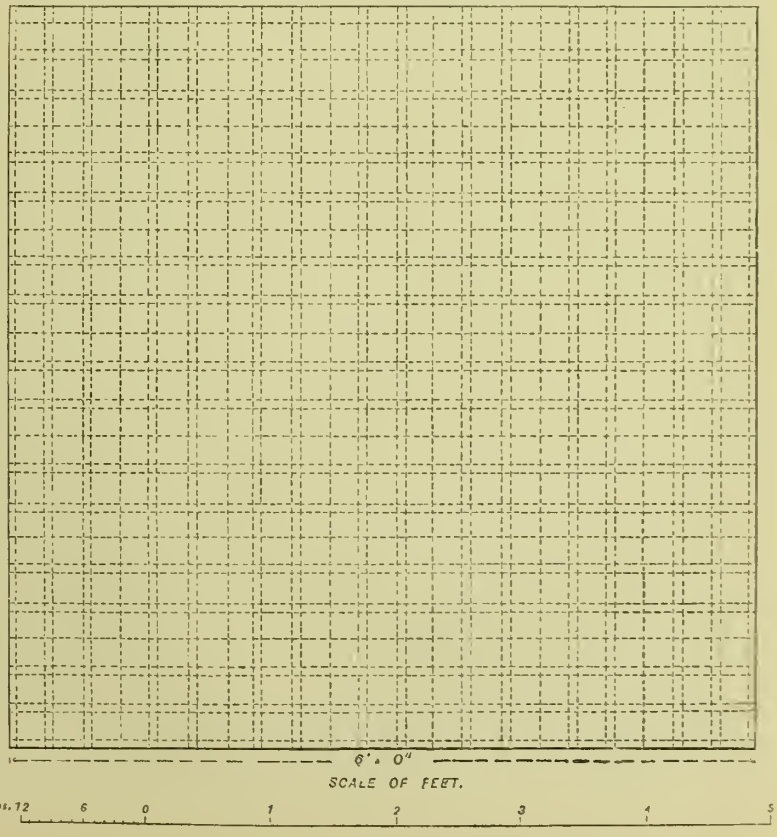


Proposed Design for Turrets of the Inflexible Class, on the principle of the Millwall Shield, with Stringers running vertically and horizontally.

VERTICAL SECTION (9).

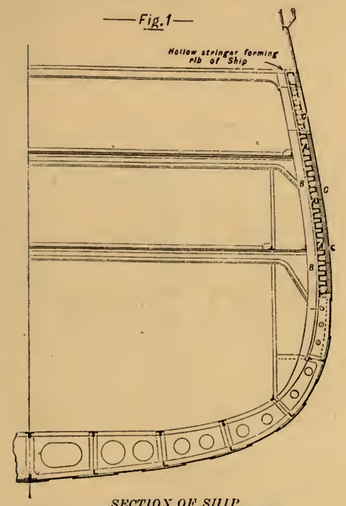


FRONT ELEVATION (8).



To illustrate Sir R. Spencer Robinson's Paper on Armour Plating Ships of War.

—Fig. 1—

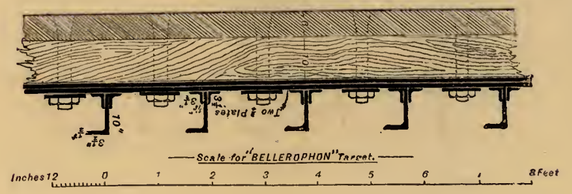


SECTION OF SHIP

Showing the manner in which Hughes' Patent Hollow Stringers are employed jointly for the construction of the vessel and the backing of the armour.

—Fig. 4—

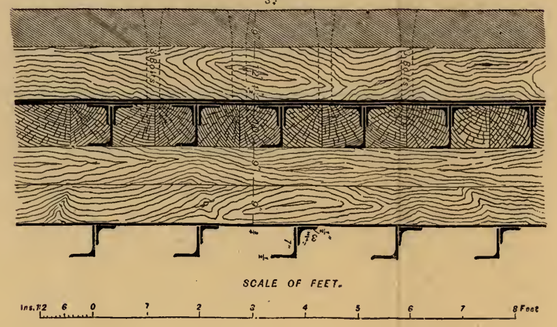
—HORIZONTAL SECTION—



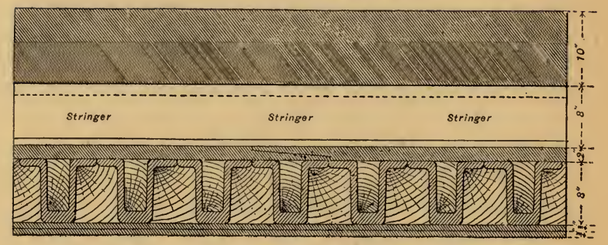
—"HERCULES" TARGET—

Constructed 1865.

—HORIZONTAL SECTION— —Fig. 6—

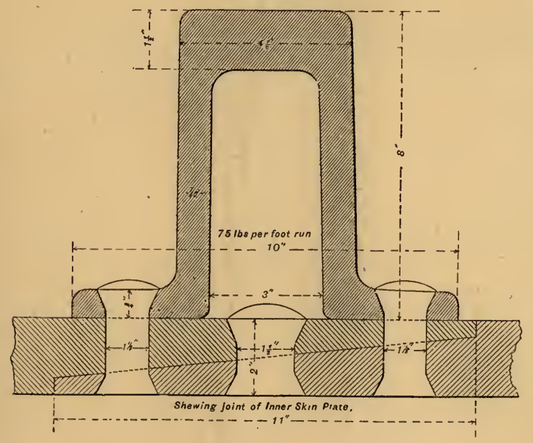


HORIZONTAL SECTION (7).



Section of Hughes' Patent Hollow Stringer as proposed for Shield represented in Figs. 7, 8, 9.

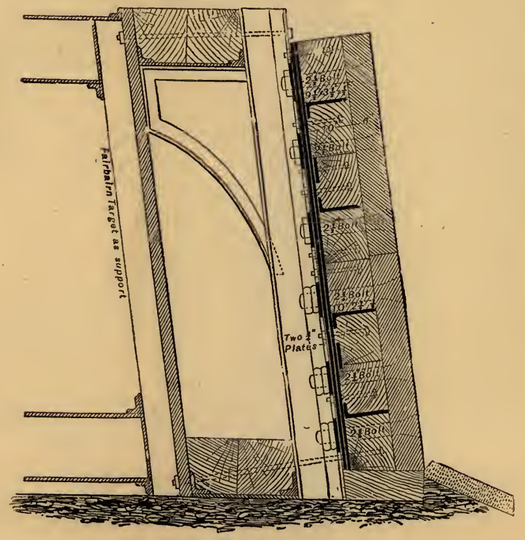
—Fig. 2—



—SECTION OF "BELLEROPHON" CONSTRUCTED 1863.—

(Fairbairn Target Shewn in Support)

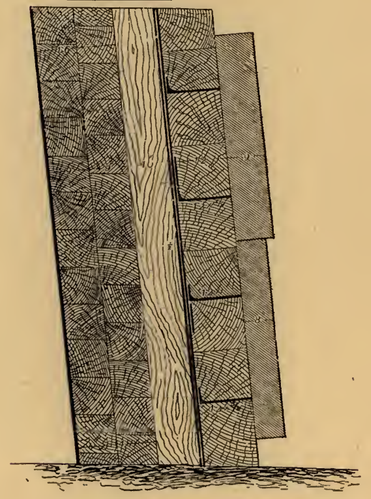
—Fig. 3—



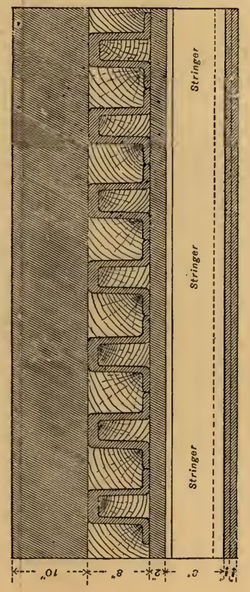
—"HERCULES" TARGET.—

—Fig. 5—

—VERTICAL SECTION—

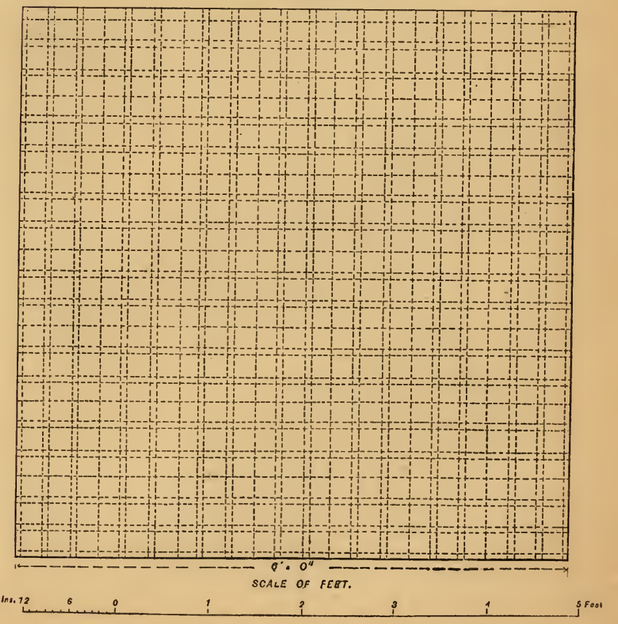


VERTICAL SECTION (9).



Proposed Design for Turrets of the Inflexible Class, on the principle of the Millwall Shield, with Stringers running vertically and horizontally.

FRONT ELEVATION (8).



To illustrate Mr. Scott Russell's Paper on the True Nature of the Wave of Translation, and the Part it Plays in Removing Water out of the Way of a Ship with Least Resistance.

DIAGRAM I. *Geometry of the Circle.*

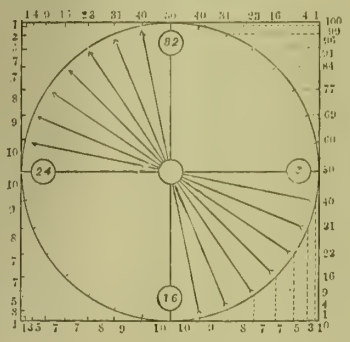


DIAGRAM II. *Diagram of Gravitation Isochronism.*

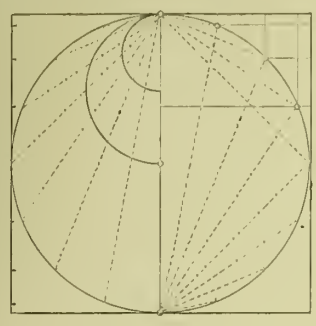


DIAGRAM V. *Isochronism Equivalent Water-Columns.*

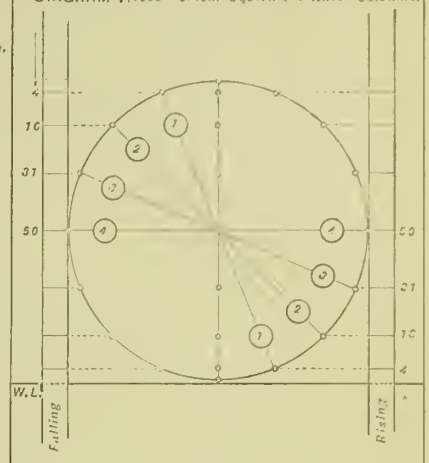


DIAGRAM III. *Diagram of Hydrodynamic Isochronism.*

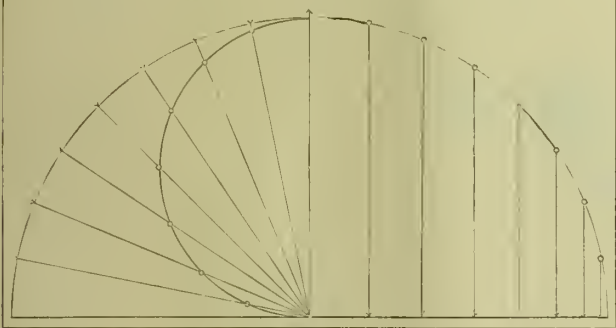


DIAGRAM IV. *Syncronism in Water Particle vertical Paths.*

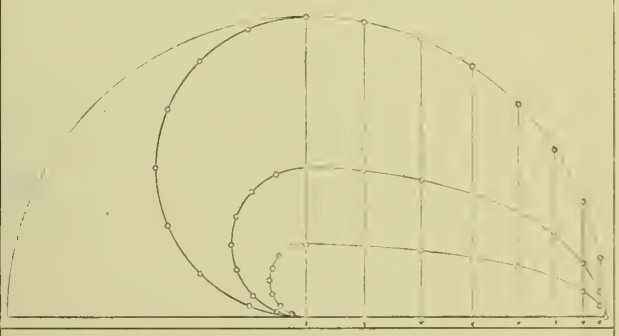


DIAGRAM VI. *Synchroric Rise and Fall from Rest to Rest.*

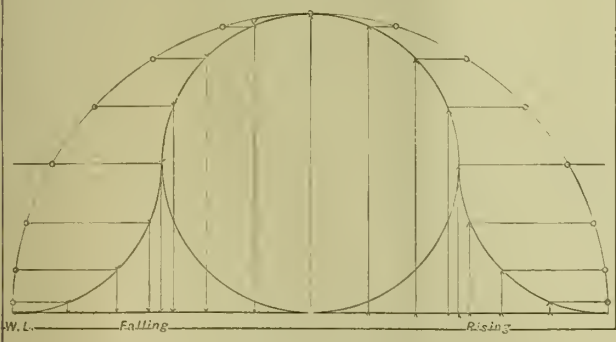


DIAGRAM VII. *Successive Motions of Water-Columns.*

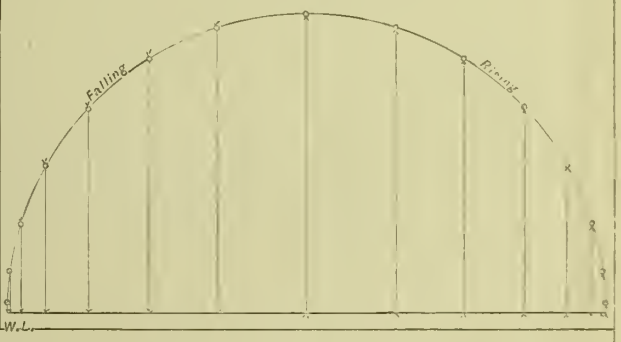
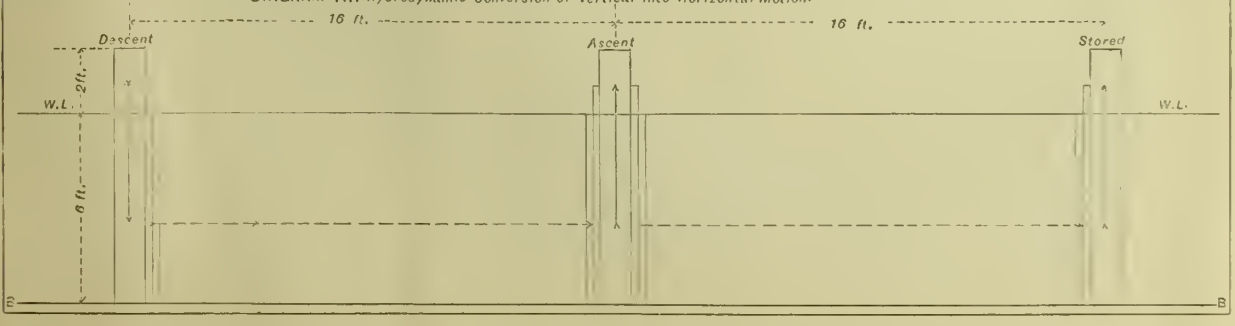
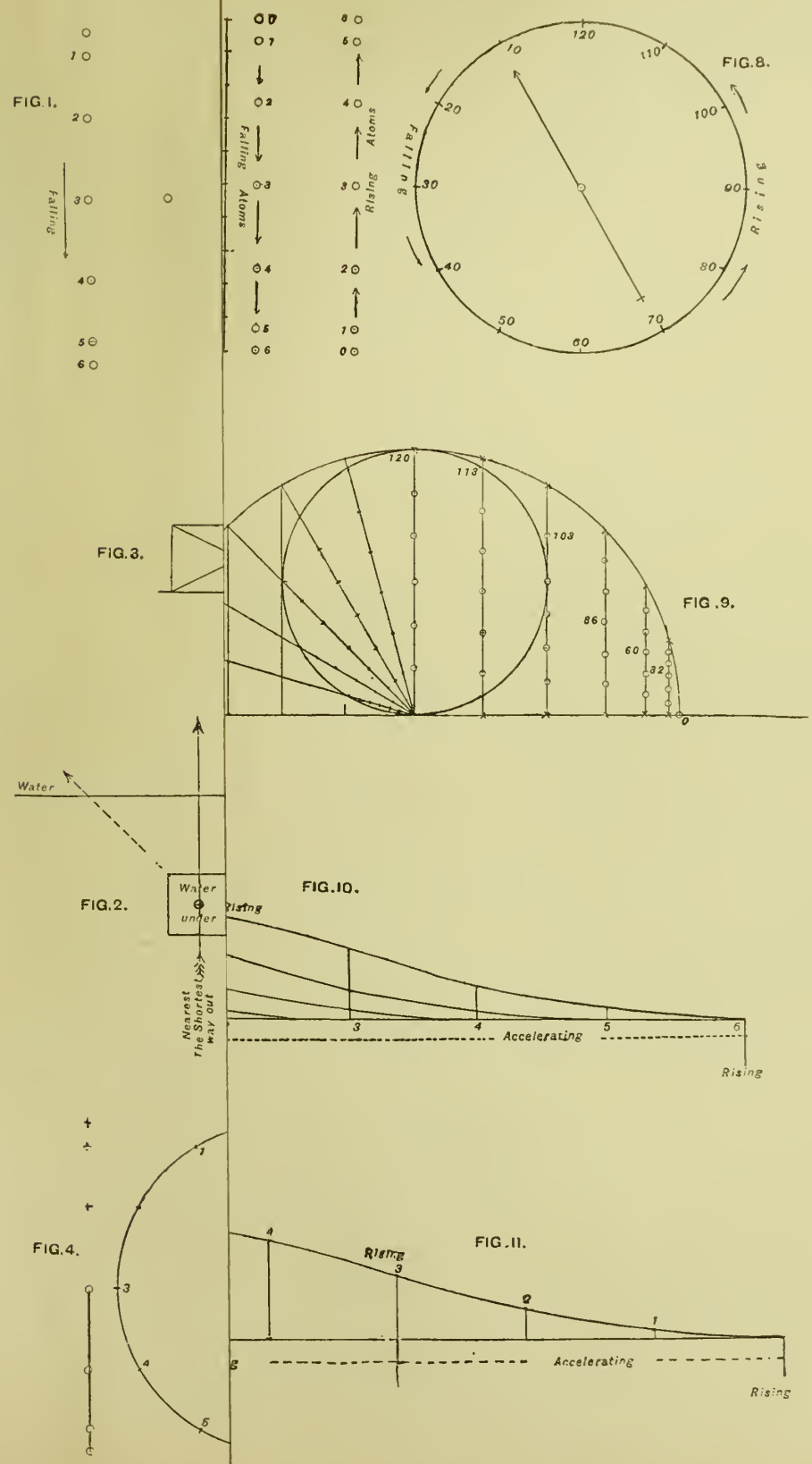


DIAGRAM VIII. *Hydrodynamic Conversion of Vertical into Horizontal Motion.*

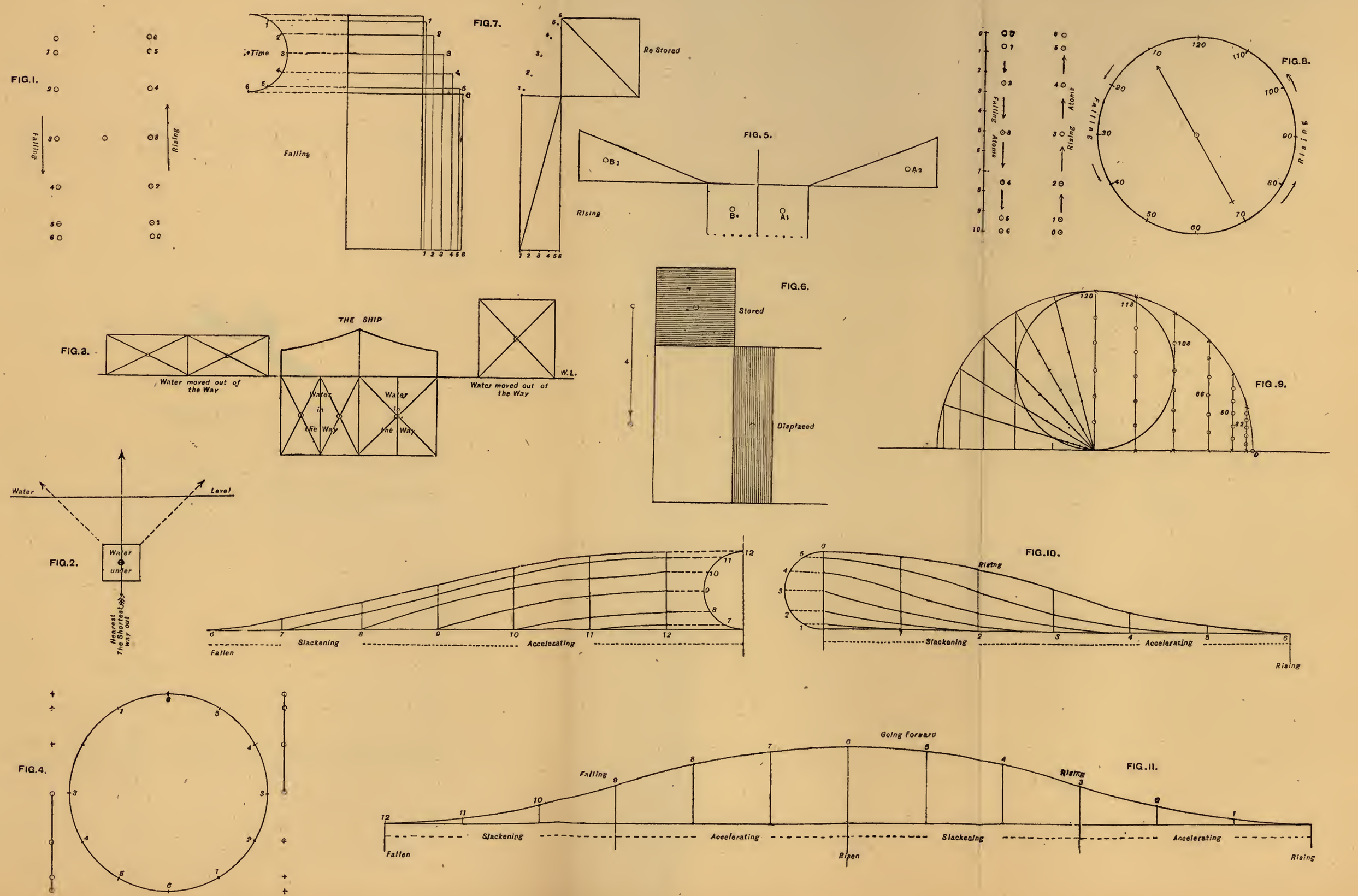


UNIVERSITY OF ILLINOIS.

To illustrate Mr. Stout of the Way of a Ship with Least Resistance



To illustrate Mr. Scott Russell's Paper on the True Nature of the Wave of Translation, and the Part it Plays in Removing Water out of the Way of a Ship with Least Resistance.



To out of the Way of a Ship with Least Resistance.

FIG 14.

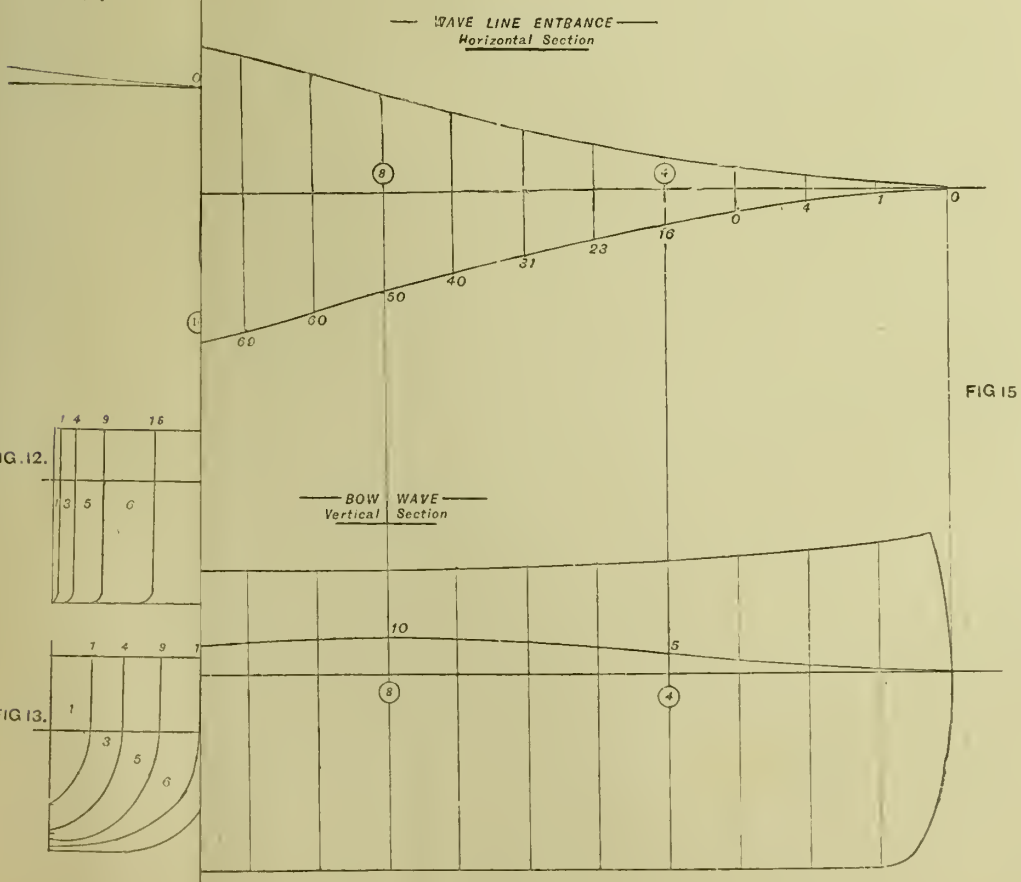
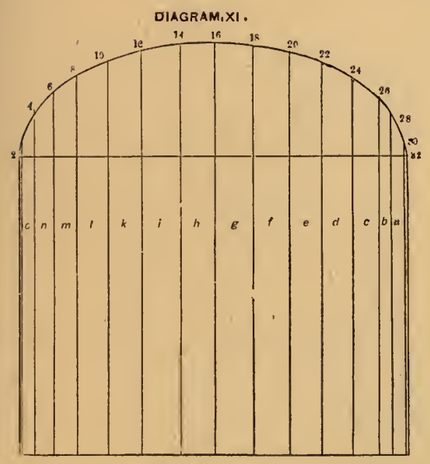


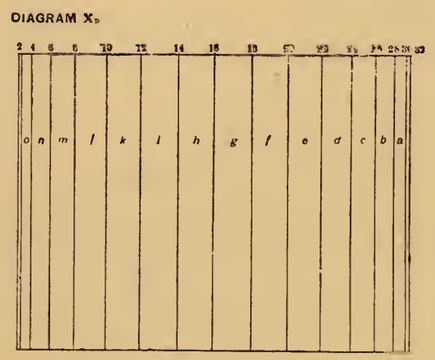
FIG 15

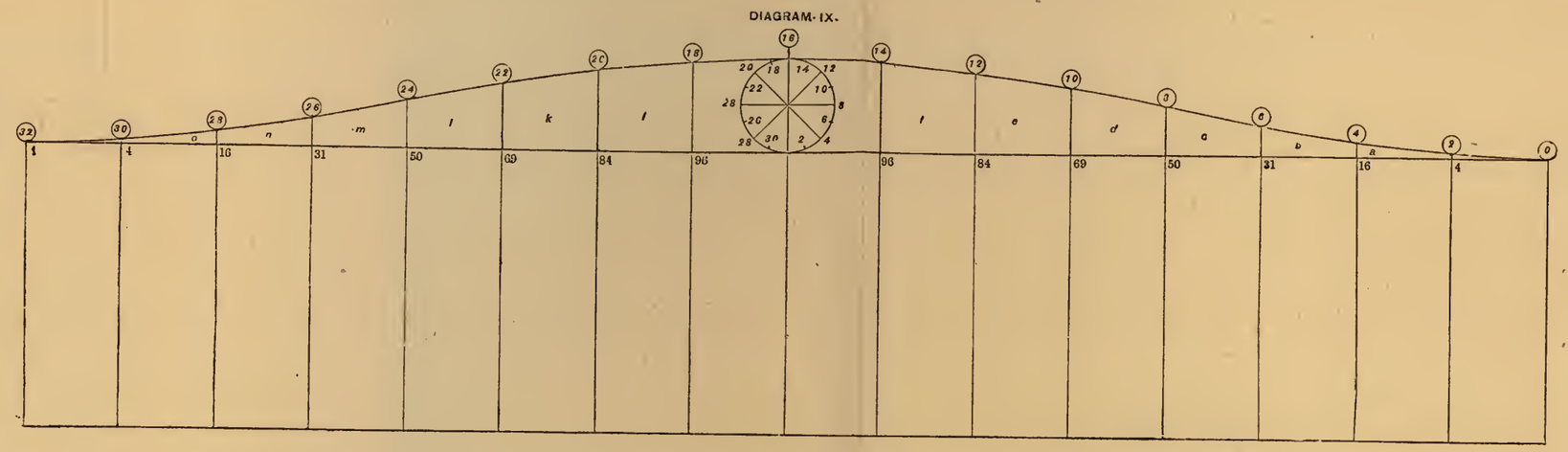
FIG. 12.

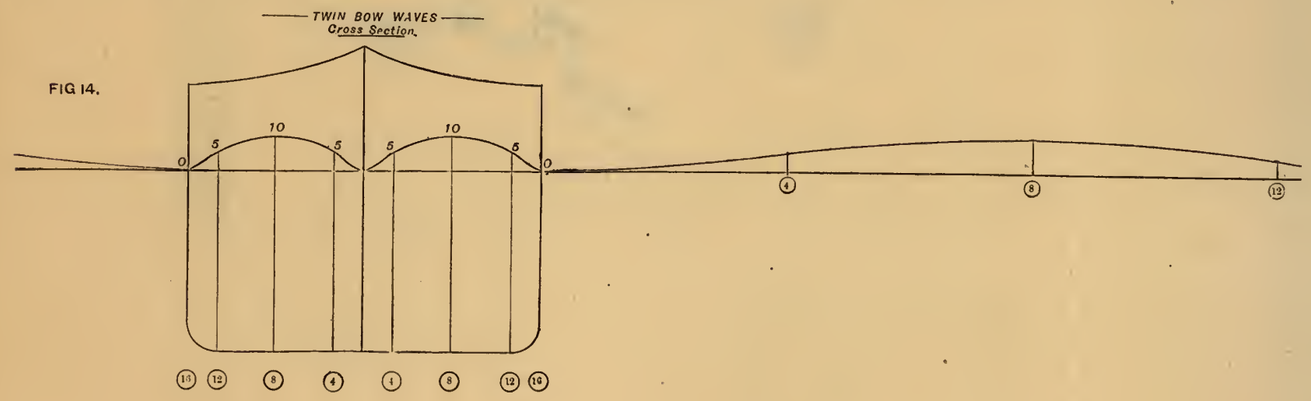
FIG 13.

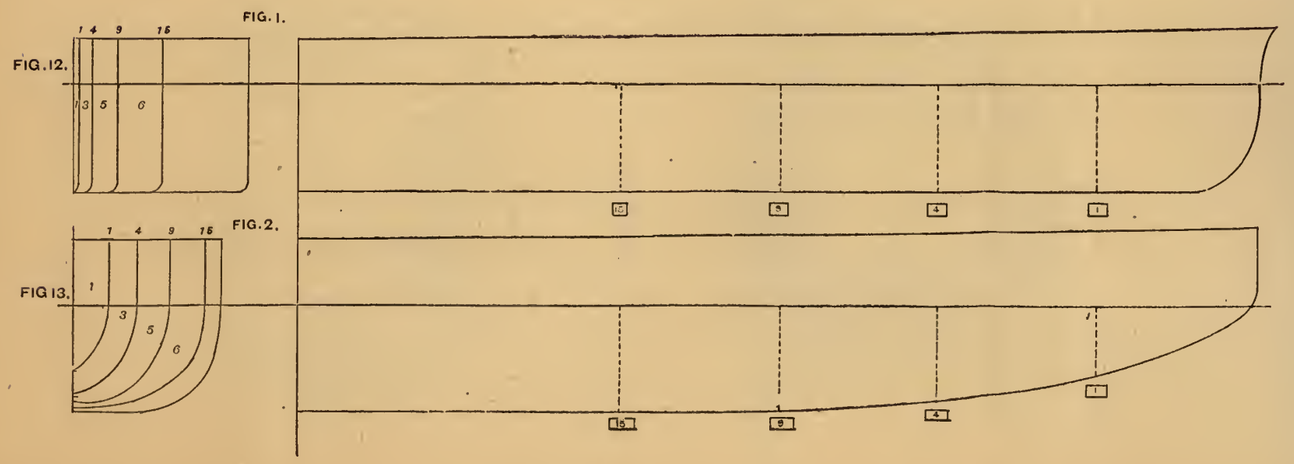
To illustrate Mr. Scott Russell's Paper on the True Nature of the Wave of Translation, and the Part it Plays in Removing Water out of the Way of a Ship with Least Resistance.











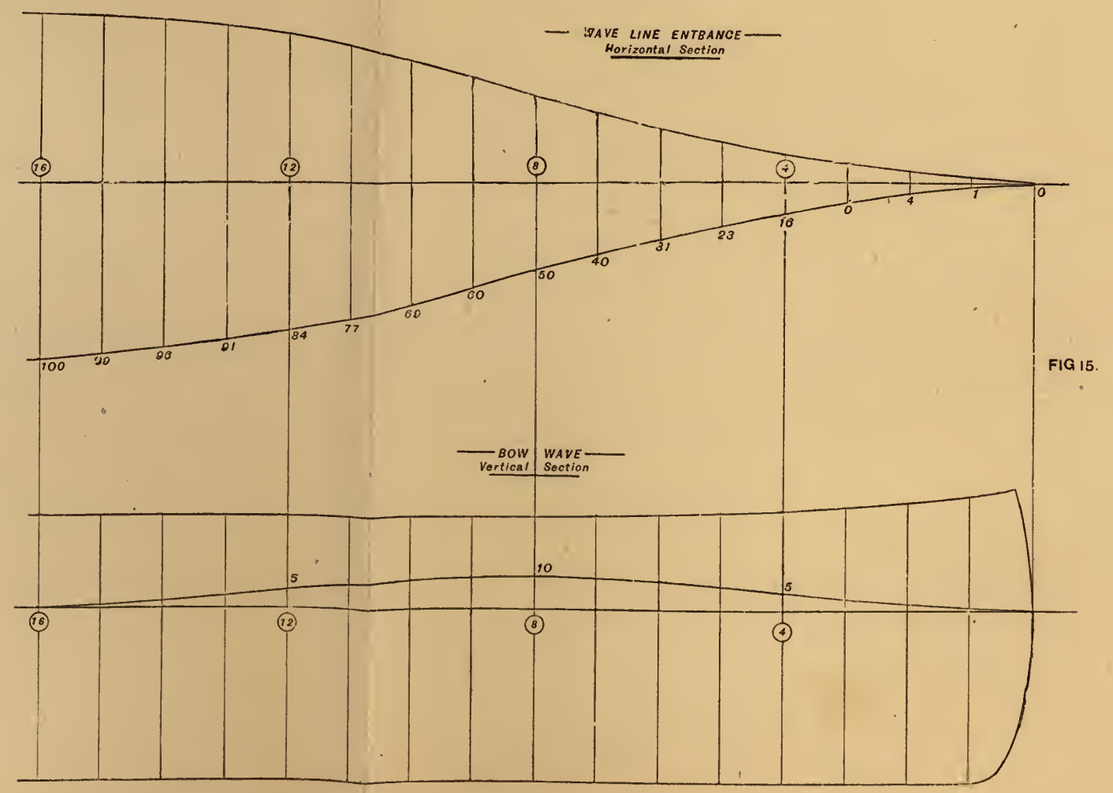


Fig. 10

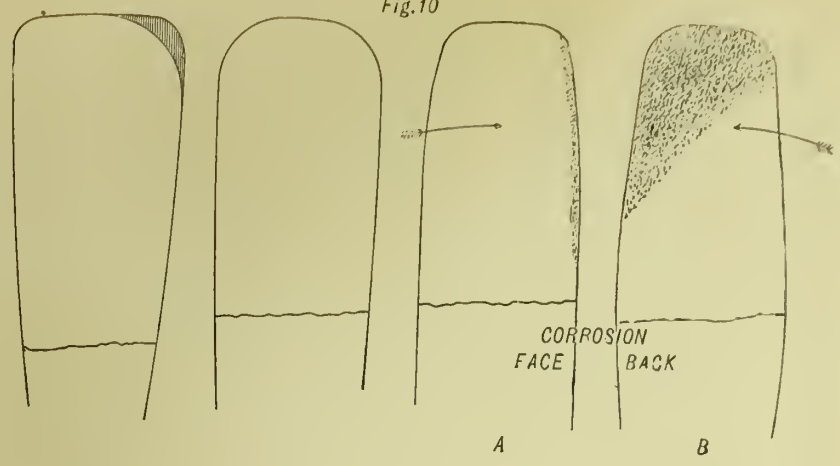
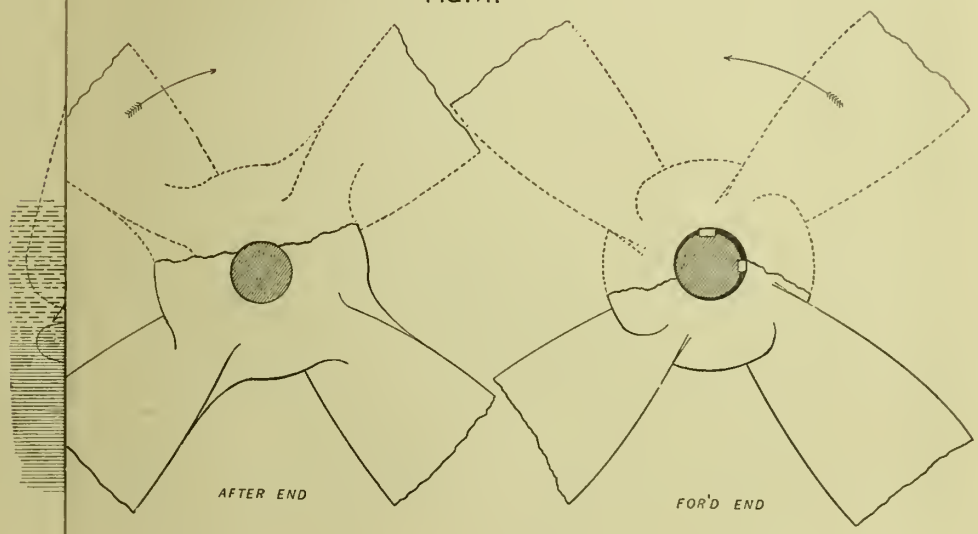


FIG. 11.



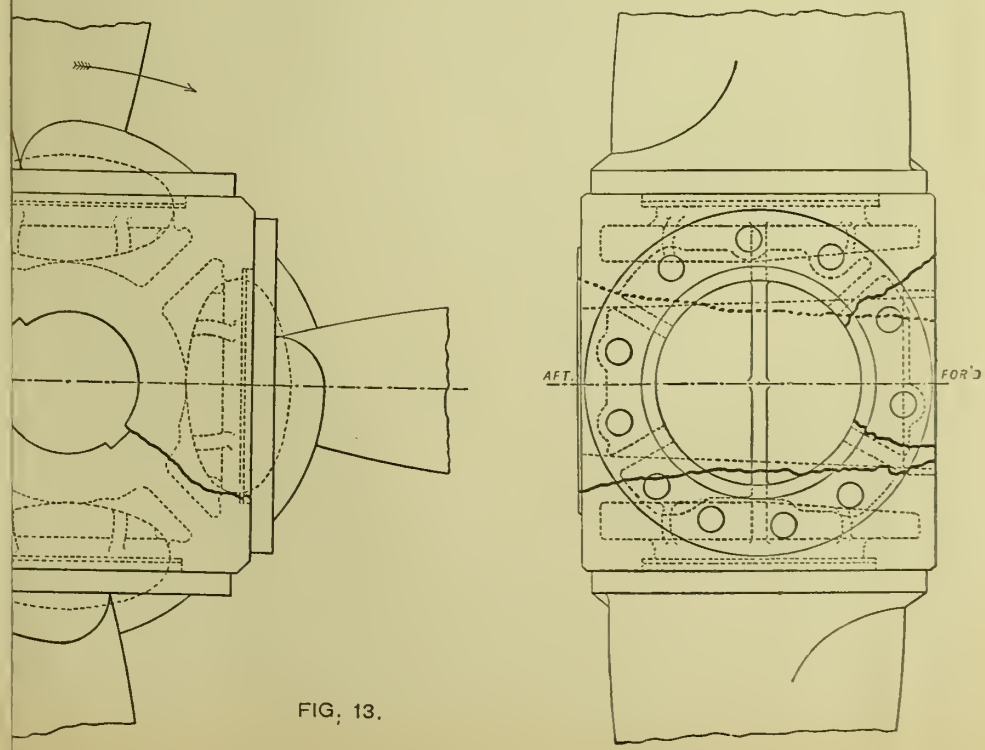
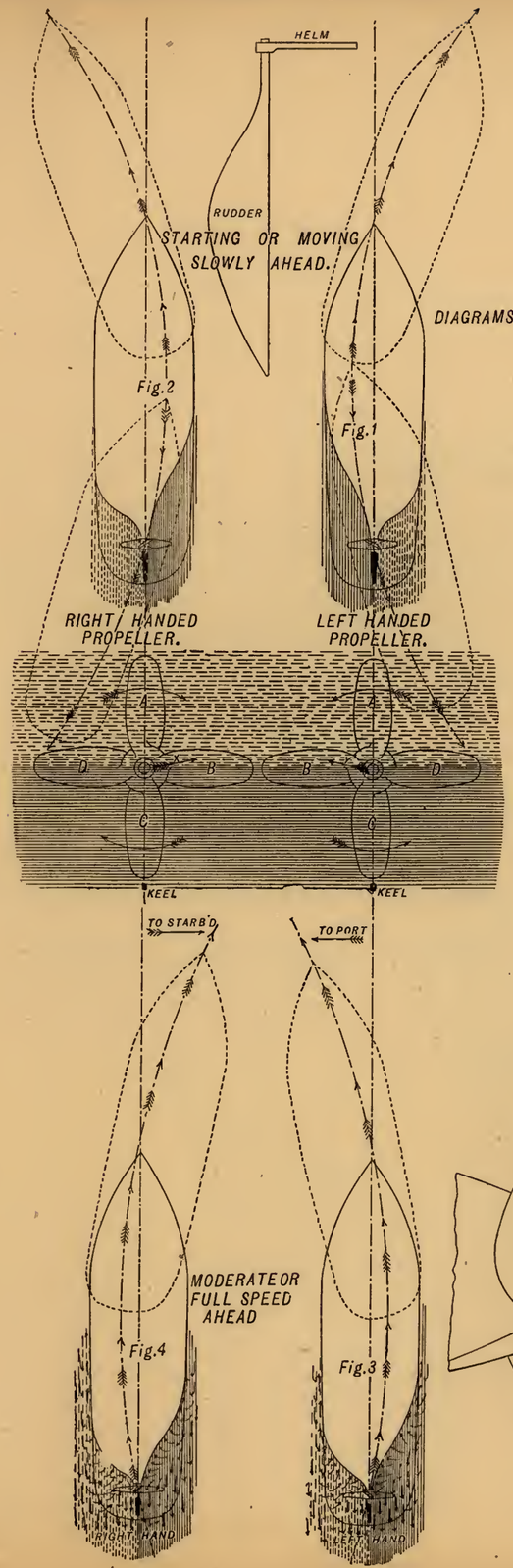
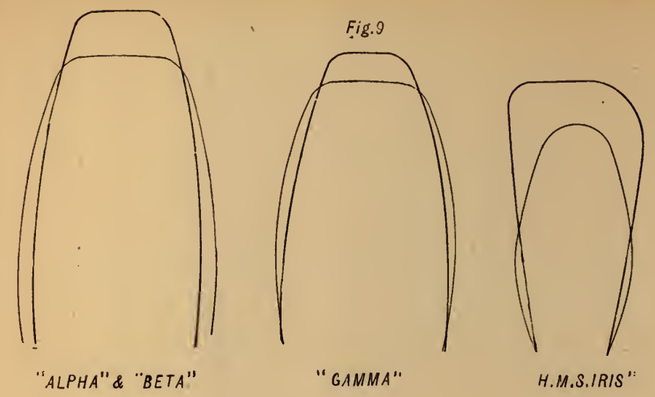


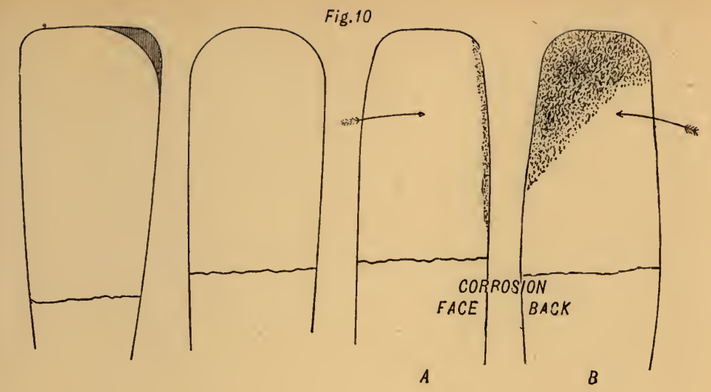
FIG. 13.

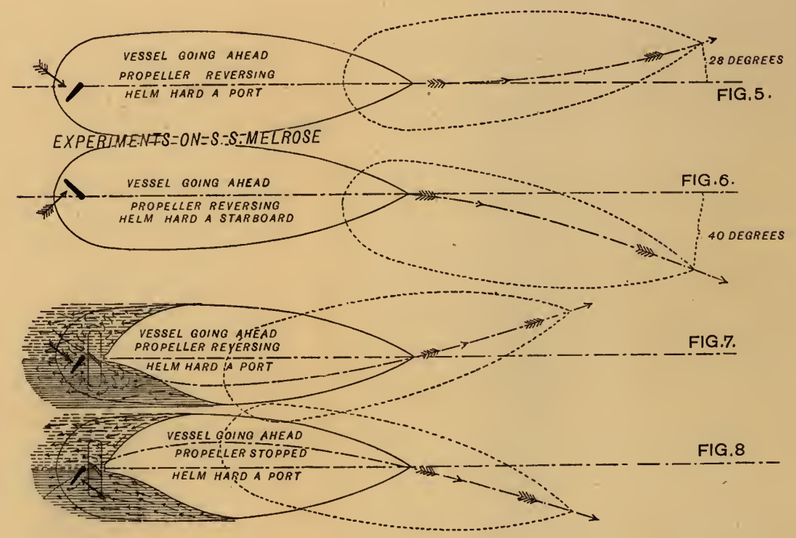
To illustrate Mr. A. J. Magnini's Paper on Screw Propulsion.

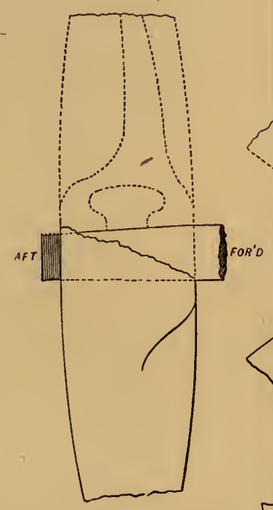


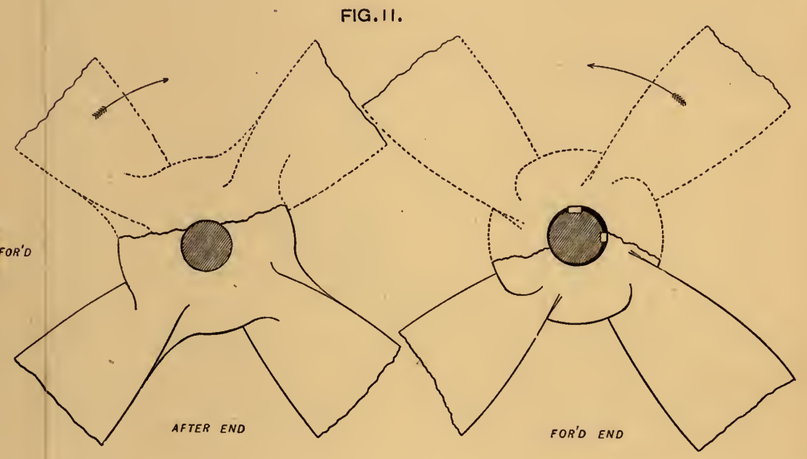
DIAGRAMS OF STEERING OF SCREW STEAMERS.











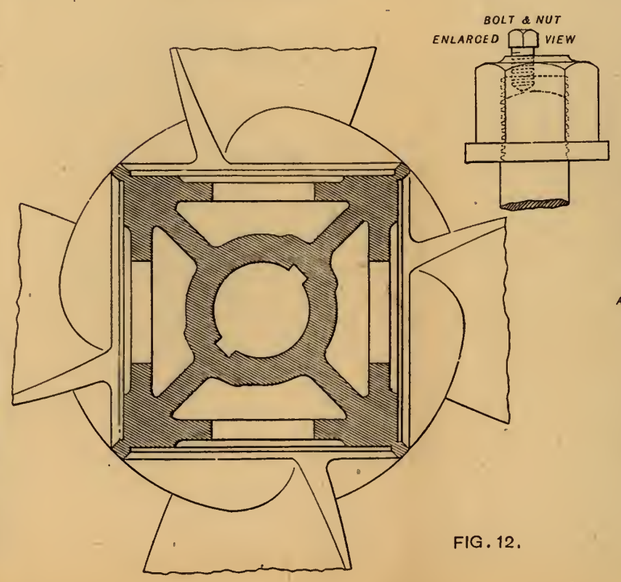
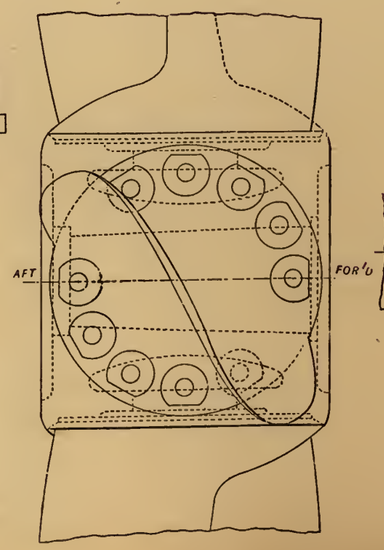


FIG. 12.



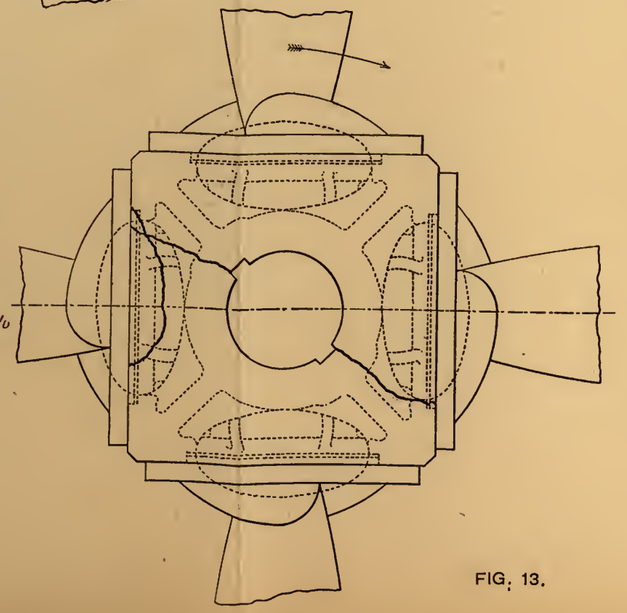
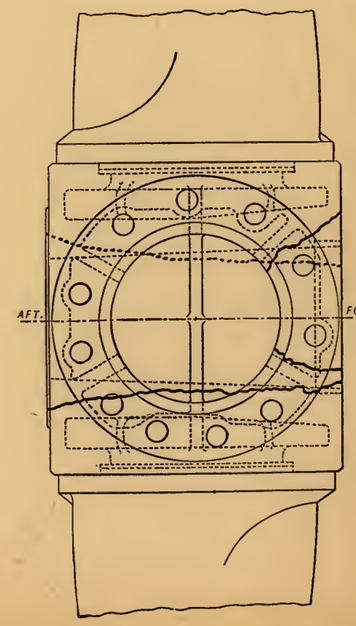
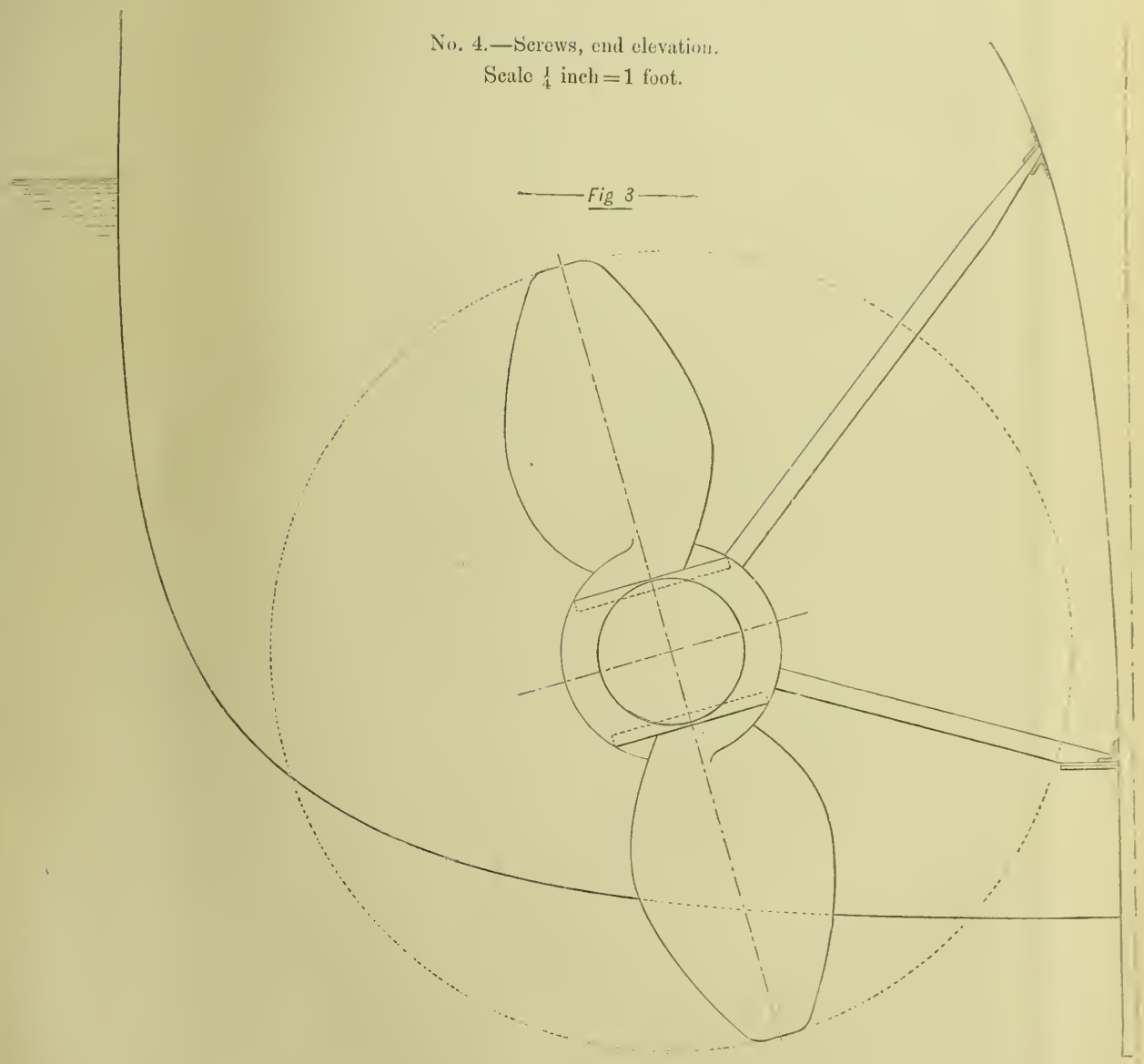


FIG. 13.



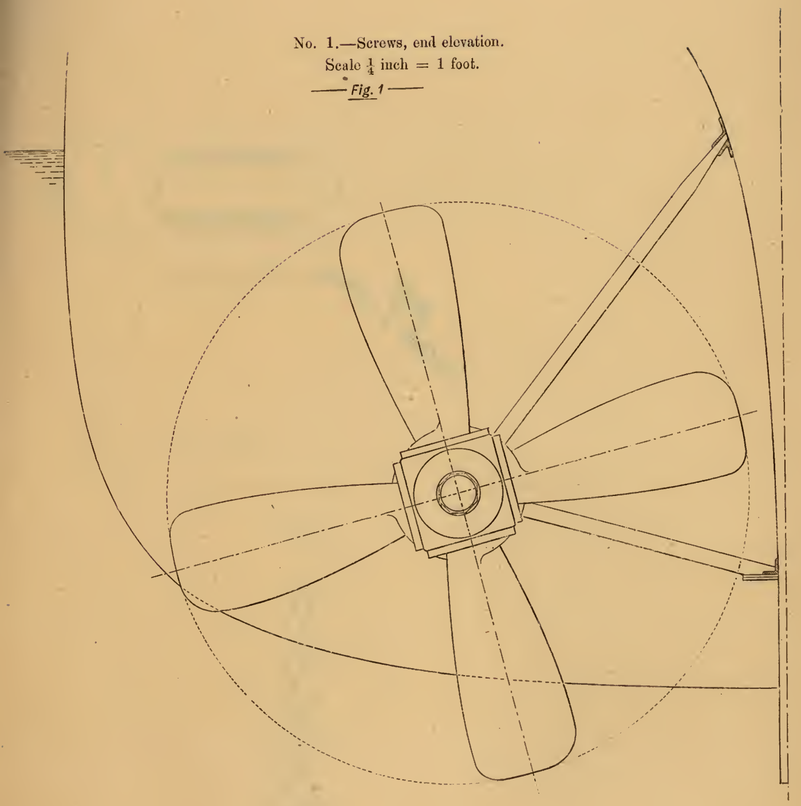
No. 4.—Screws, end elevation.
Scale $\frac{1}{4}$ inch = 1 foot.



To illustrate Mr. J. Wright's Paper on the Steam Trials of H.M.S. "Iris."

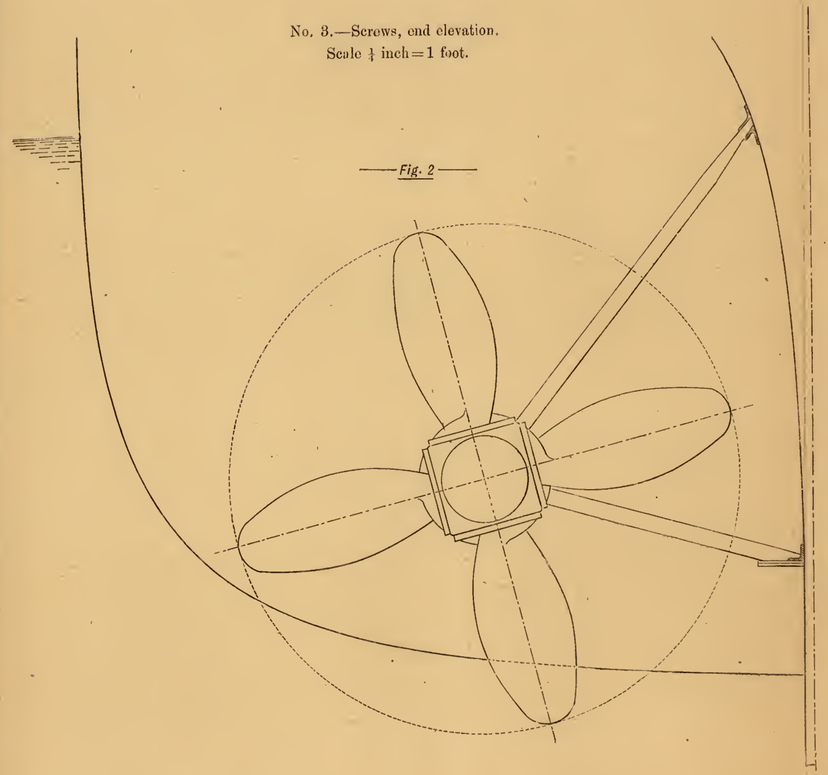
No. 1.—Screws, end elevation.
Scale $\frac{1}{4}$ inch = 1 foot.

Fig. 1



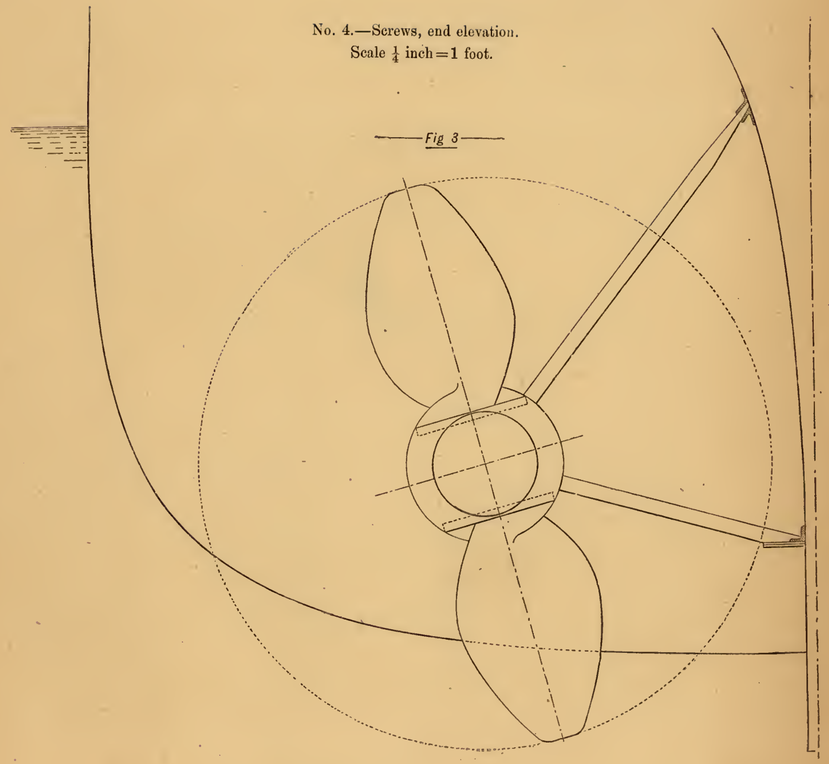
No. 3.—Screws, end elevation.
Scale $\frac{1}{4}$ inch = 1 foot.

Fig. 2



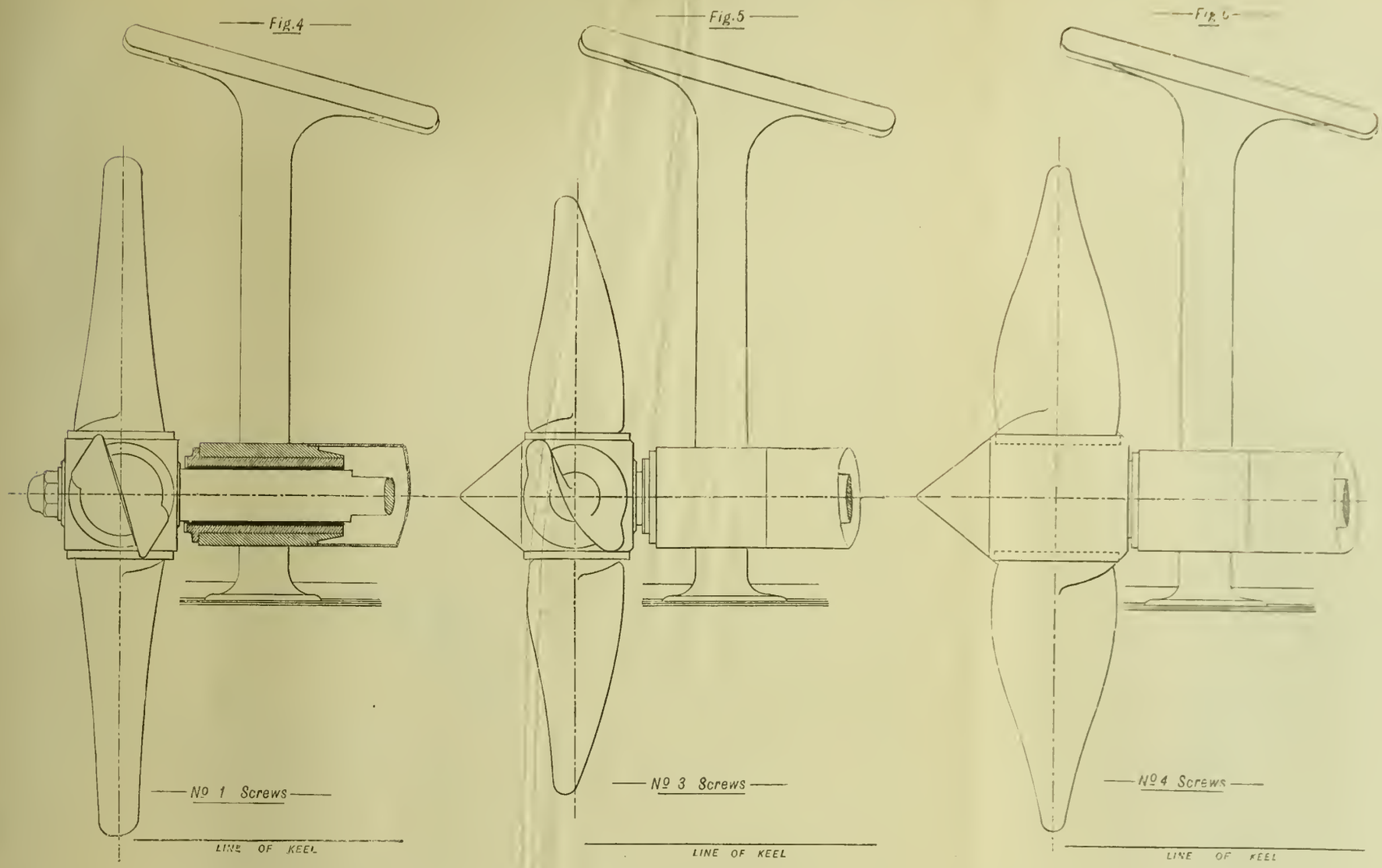
No. 4.—Screws, end elevation.
Scale $\frac{1}{4}$ inch = 1 foot.

Fig 3

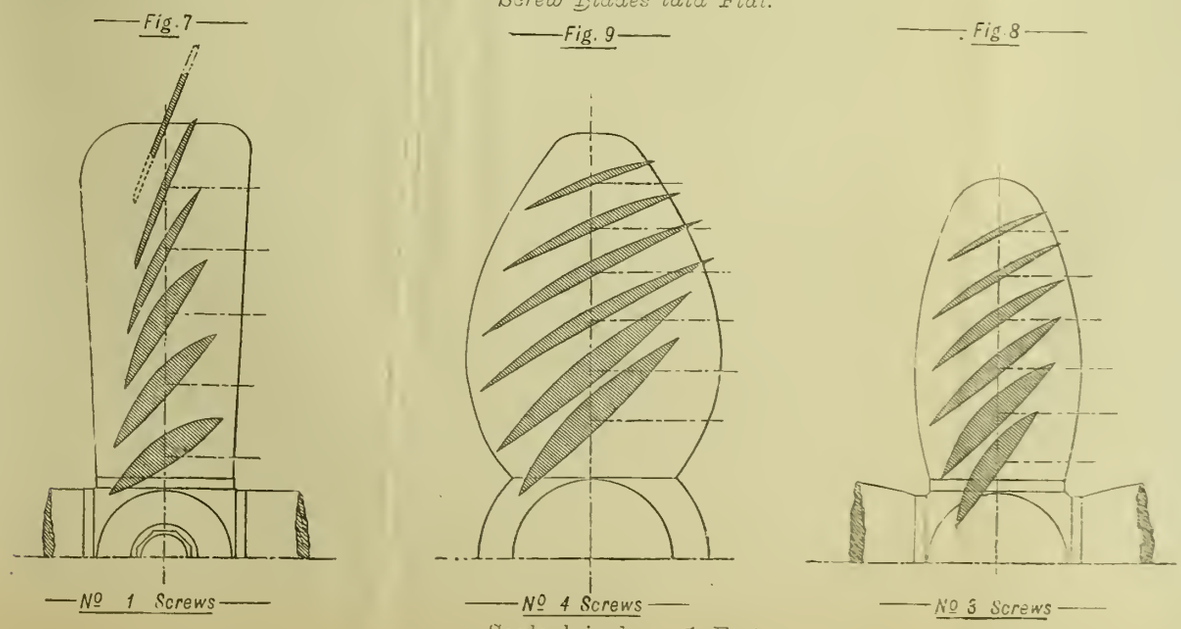


To illustrate Mr. J. Wright's Paper on the Steam Trials of H.M.S. "Iris."

Side Elevation of Screws.



Screw Blades laid Flat.



Scale ½-inch = 1 Foot.

LIBRARY

Fig. 12

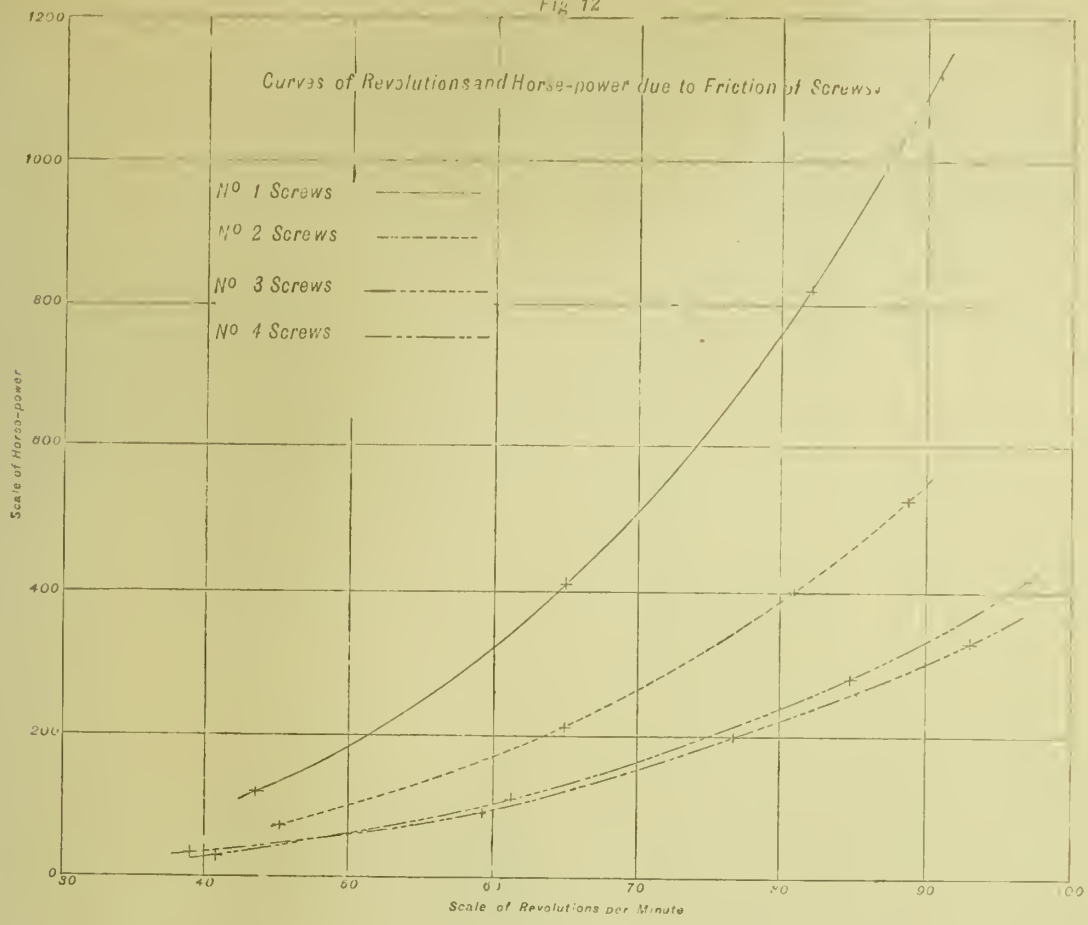
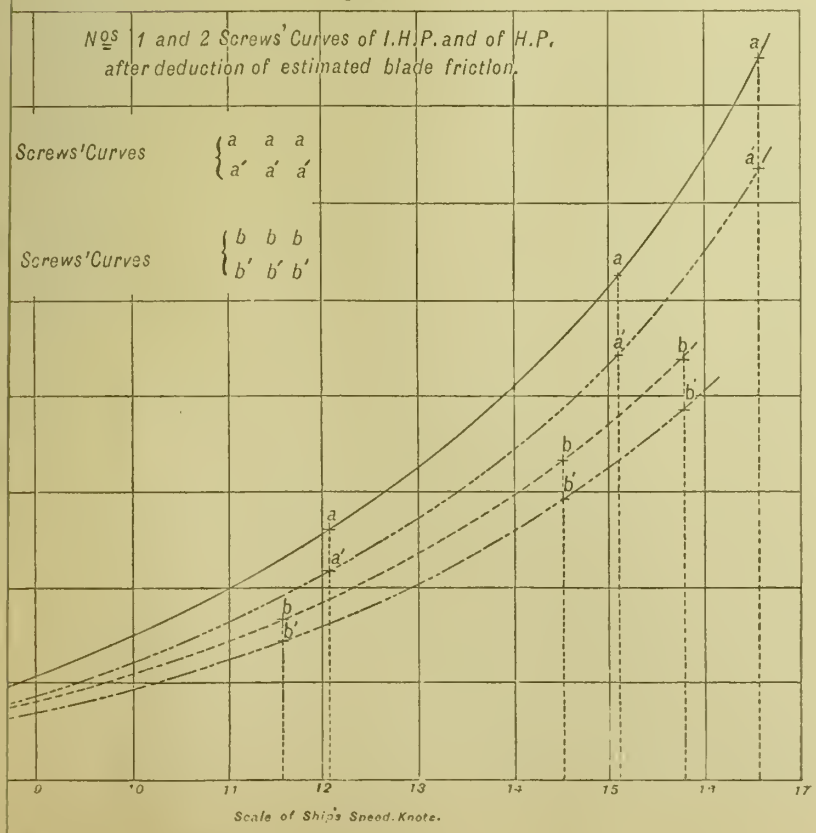

Curves of Revolutions and Horse-power due to Friction of Screws
No 1 Screws
No 2 Screws
No 3 Screws
No 4 Screws
Scale of Horse-power
Scale of Revolutions per Minute
1200
1000
800
600
400
200
0
30
40
60
70
90
100

Fig. 14.


Nos 1 and 2 Screws' Curves of I.H.P. and of H.P. after deduction of estimated blade friction.
Screws' Curves a a a a′ a′ a′
Screws' Curves b b b b′ b′ b′
Scale of Ship's Speed. Knots.
0
10
11
12
13
14
15
16
17

To illustrate Mr. J. Wright's Paper on the Steam Trials of H.M.S. "Iris."

Fig. 10

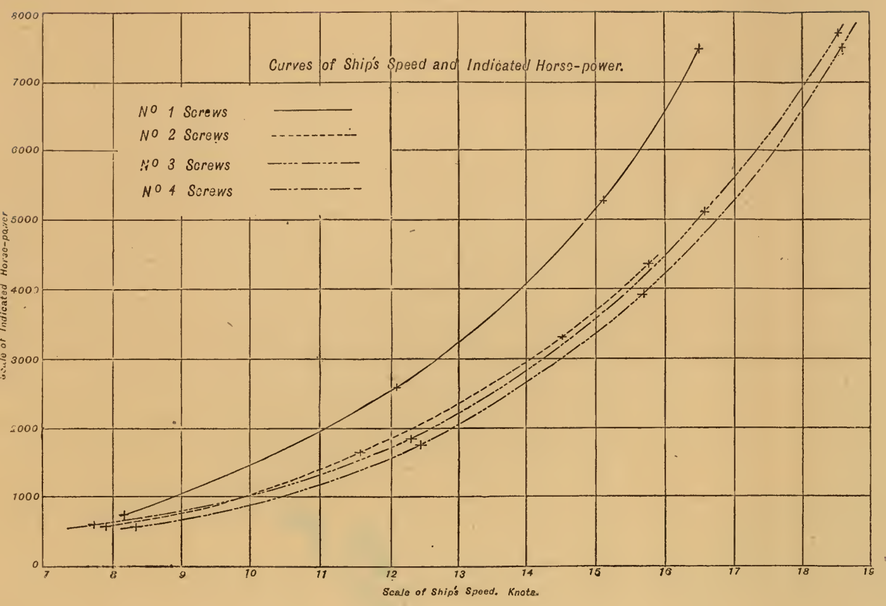


Fig. 11

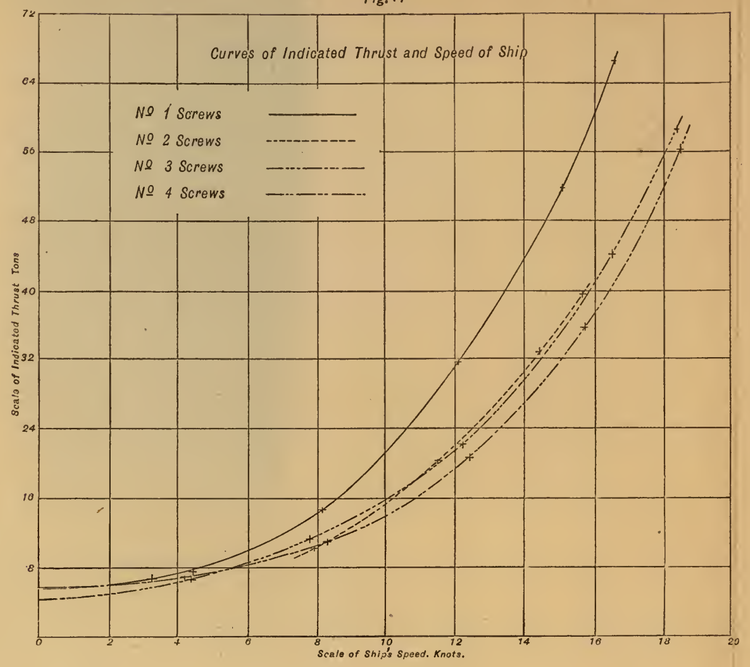


Fig 12

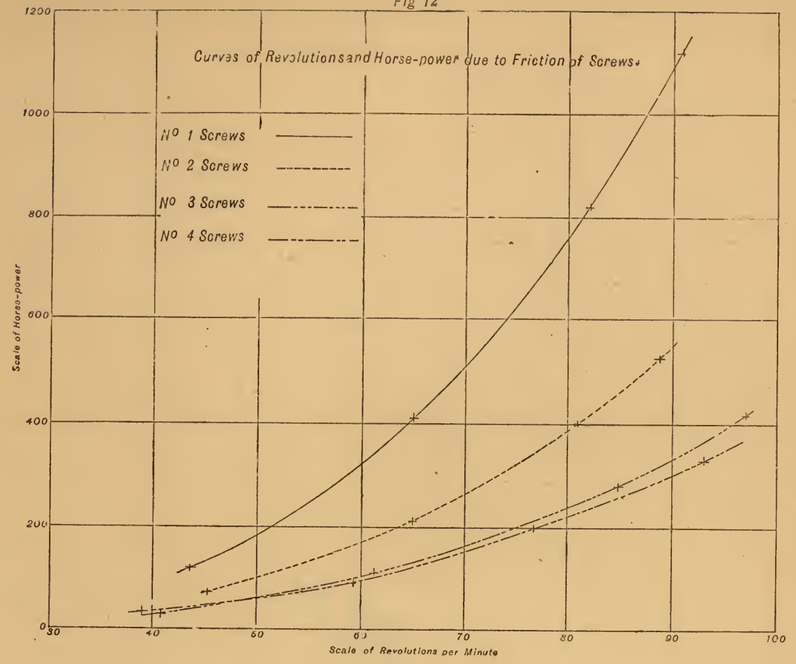


Fig 13

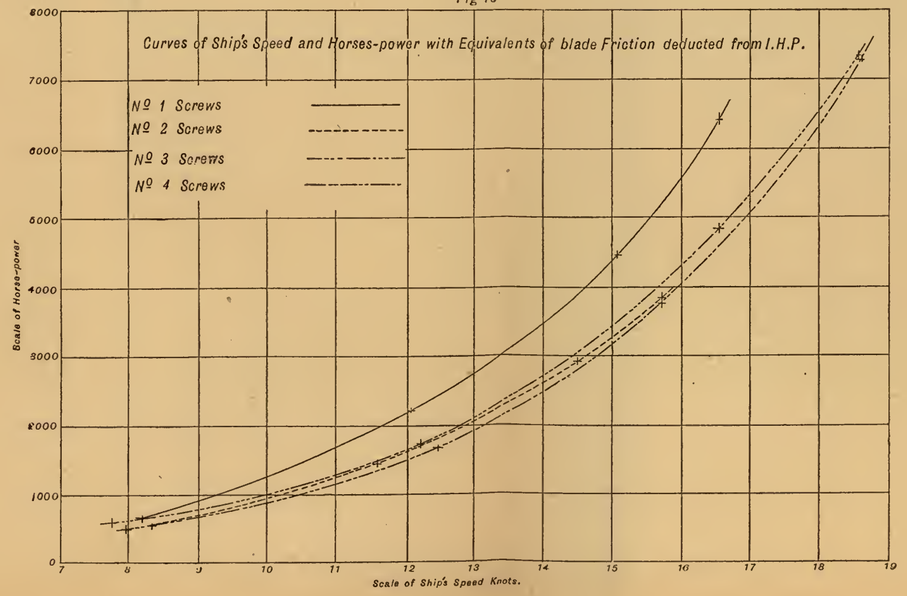
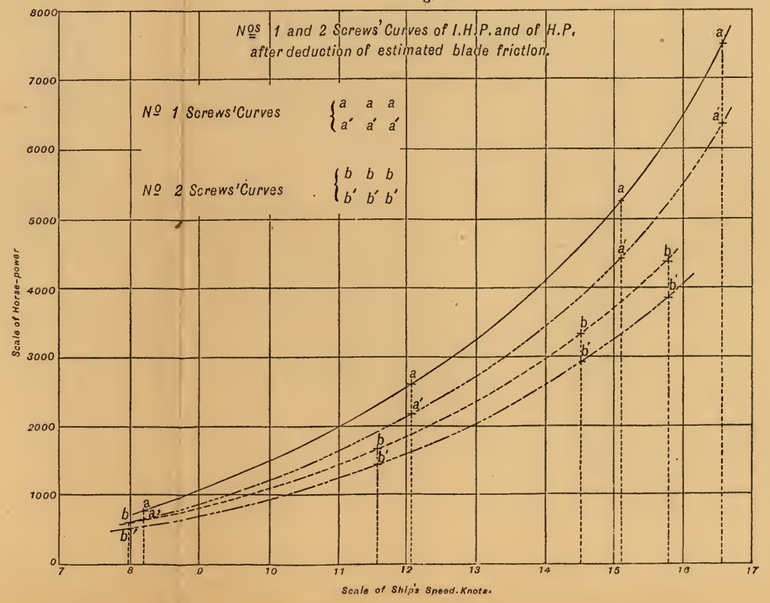


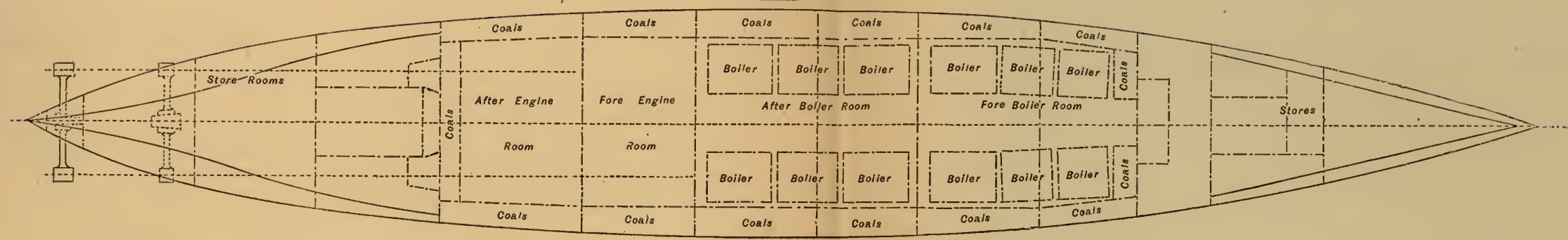
Fig. 14.



To illustrate Mr. W. H. White's Paper on the Structural Arrangements and Proportions of H.M.S. "Iris."

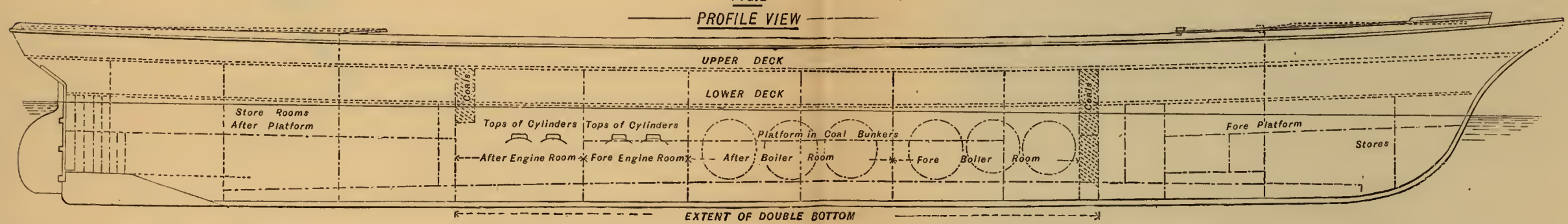
—FIG. 1—

PLAN

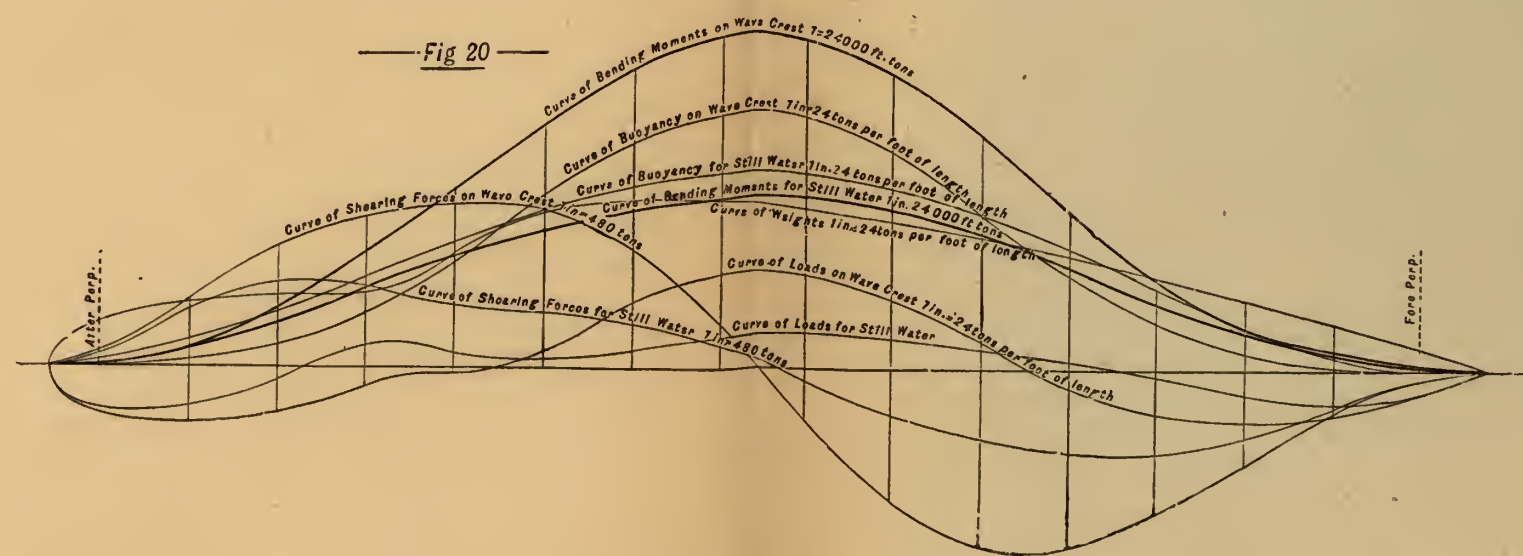


—FIG. 2—

PROFILE VIEW



—Fig 20—



To illustrate Mr. W. H. White's Paper on the Structural Arrangements and Proportions of H.M. Ships.

FIG. 4

Section abaft Double Bottom

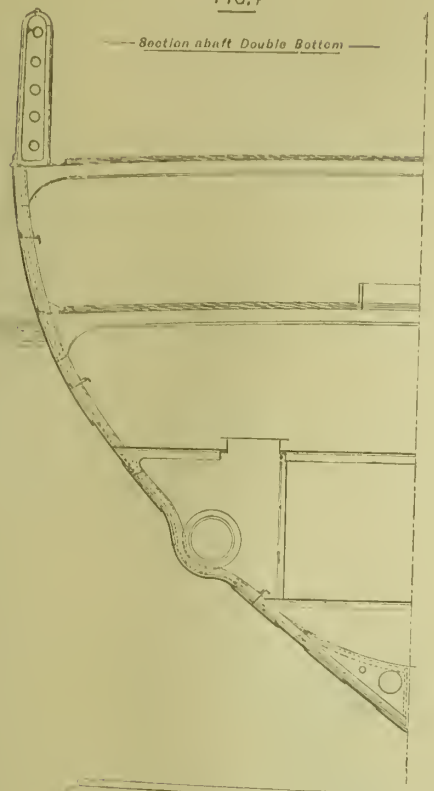


FIG. 3

Midship Section

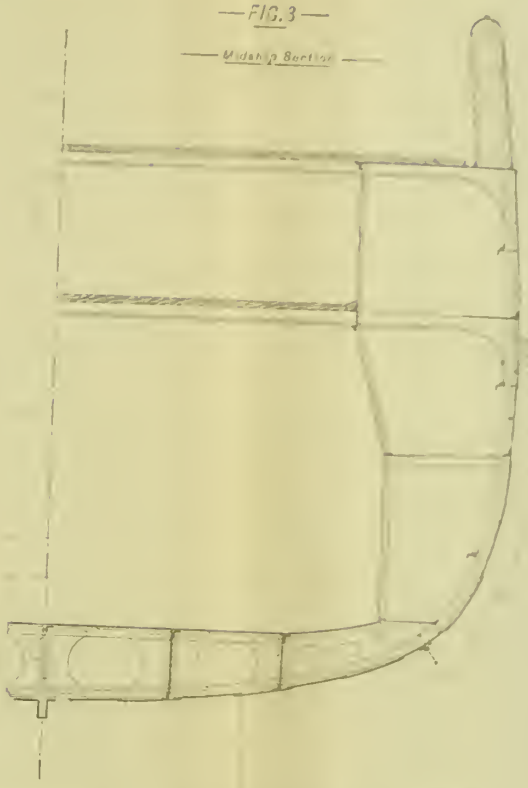
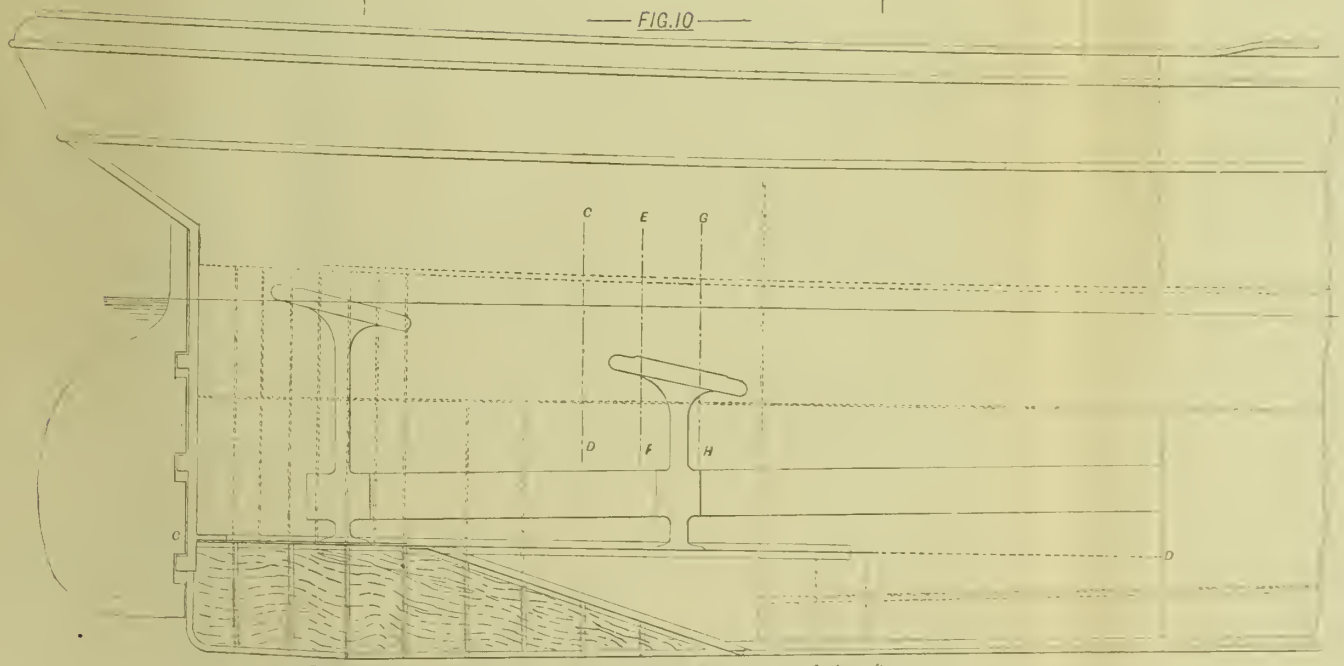
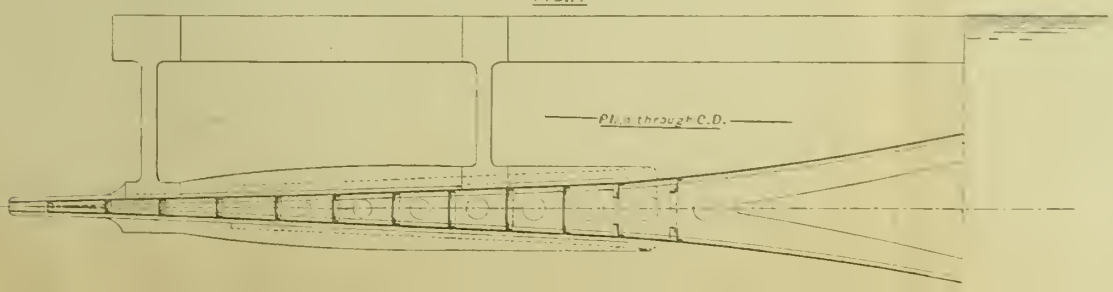


FIG. 10



Elevation. Shewing mode of securing Struts & forming fine part of ship aft

FIG. 11



Plan through C.D.

M.S. "Iris."

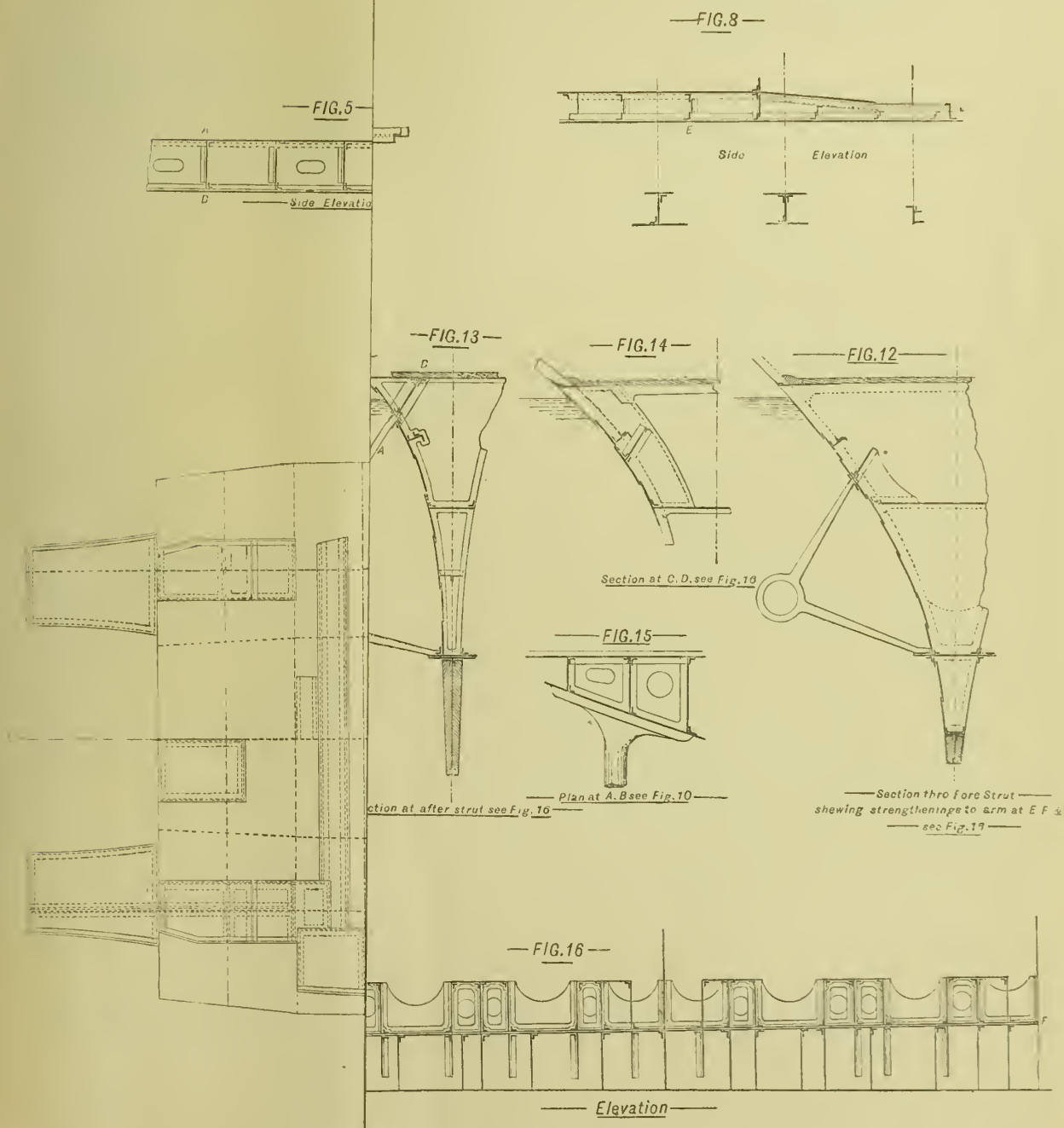
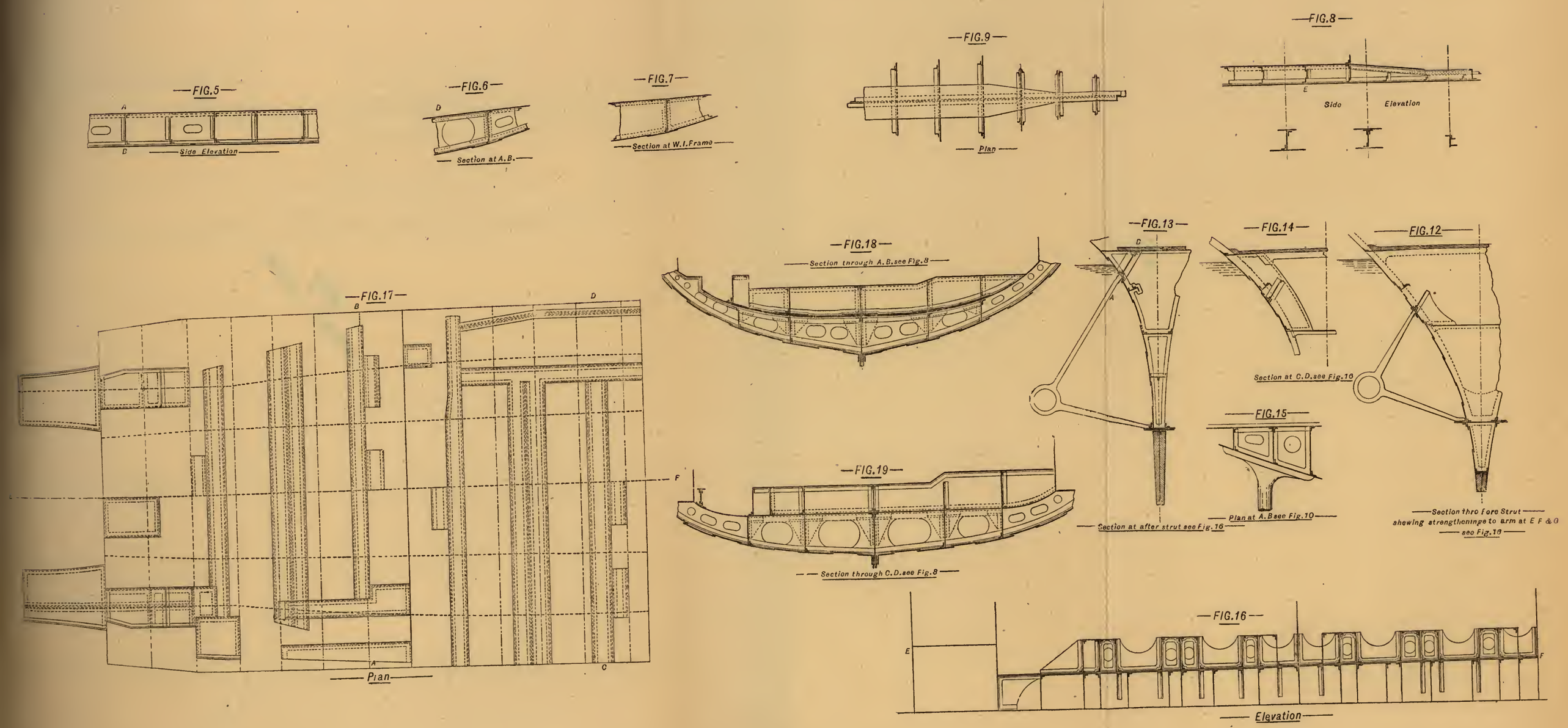
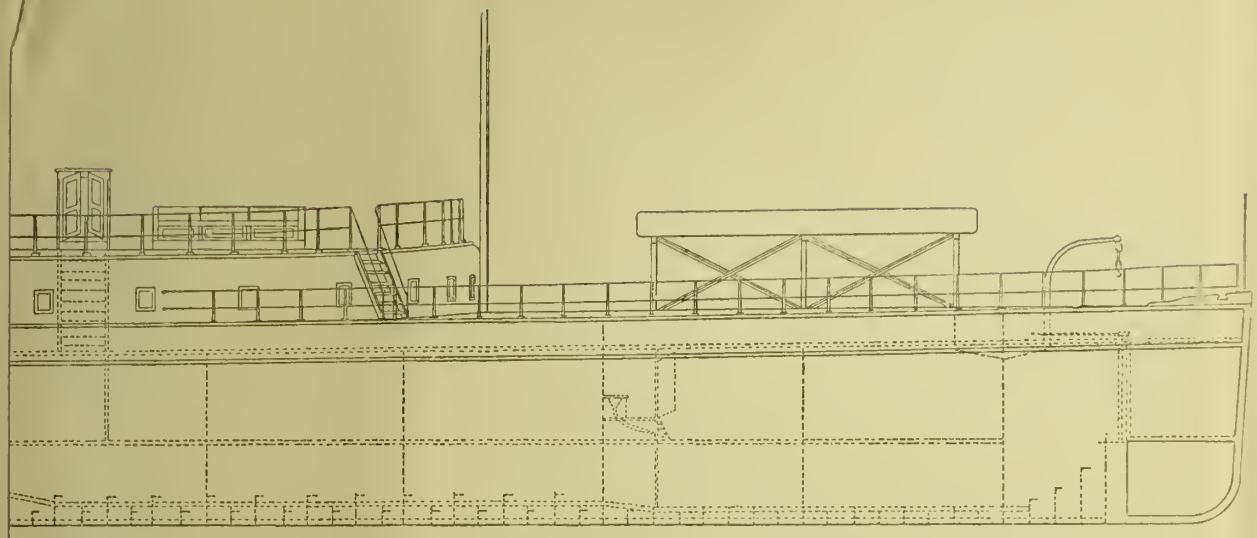
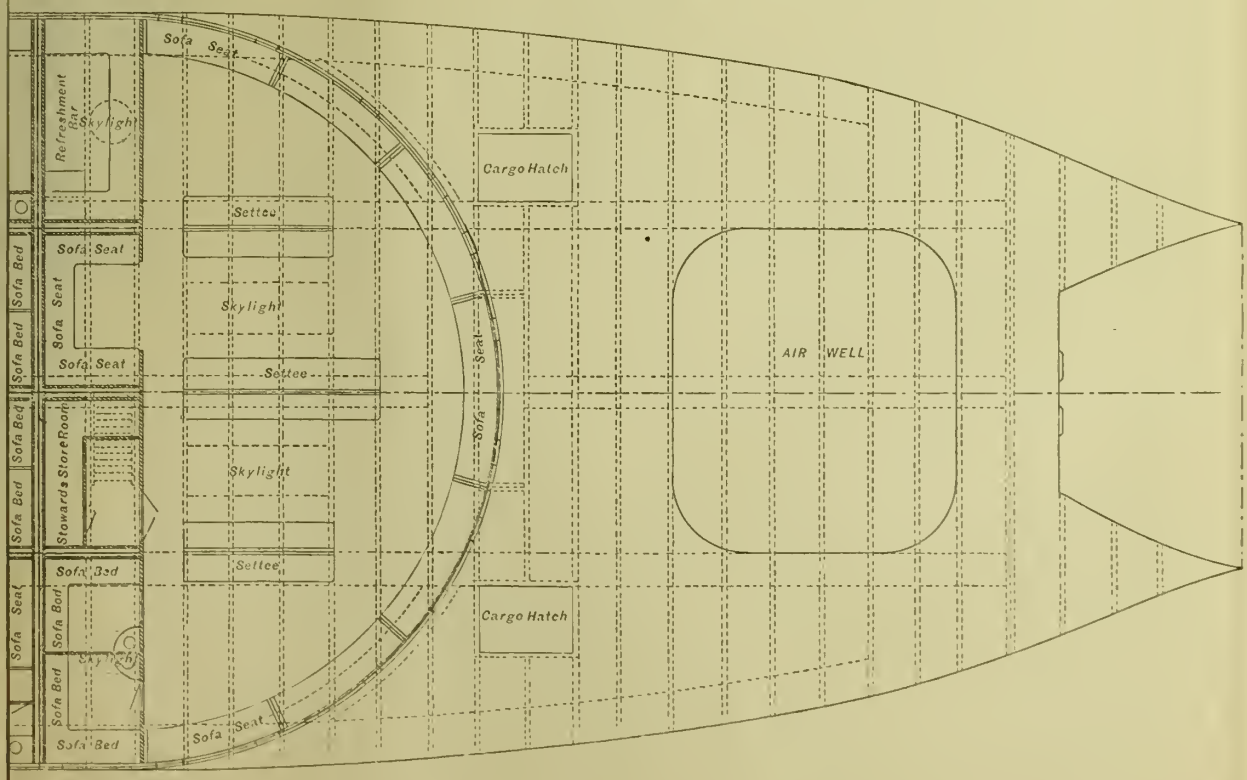


FIG.8
Side Elevation
FIG.5
Side Elevation
FIG.13
FIG.14
FIG.12
Section at C.D. see Fig.10
FIG.15
Plan at A.B see Fig.10
Section thro' fore Strut
shewing strengthenings to arm at E F &
see Fig.19
FIG.16
Elevation

To illustrate Mr. W. H. White's Paper on the Structural Arrangements and Proportions of H.M.S. "Iris."

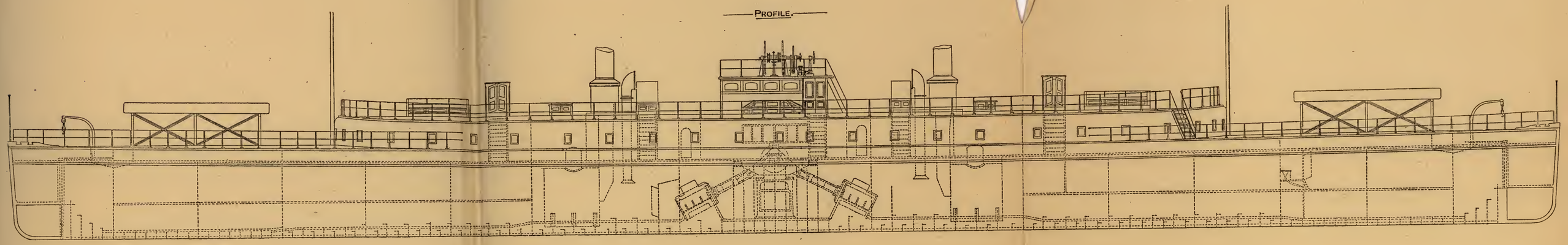




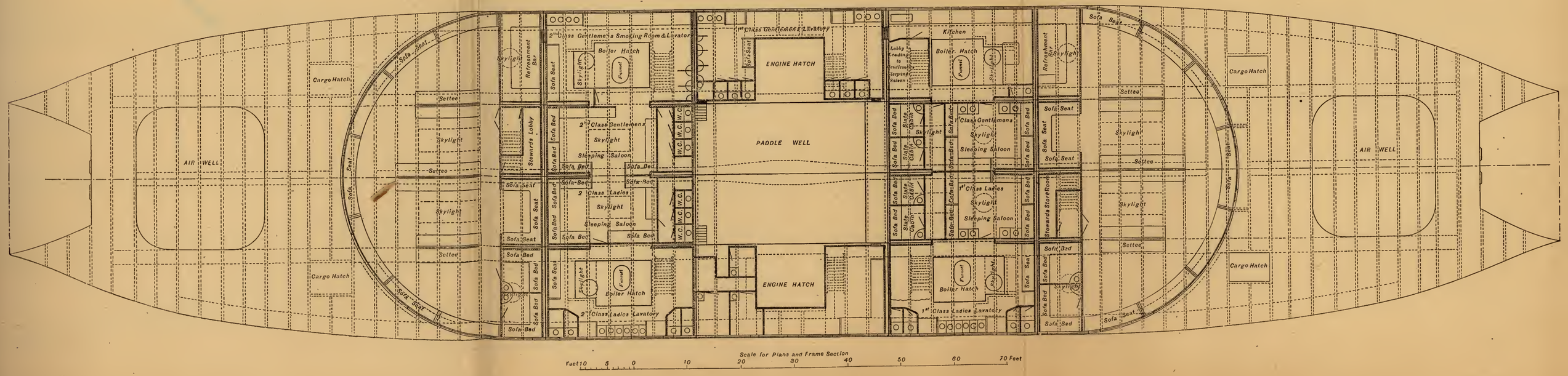

Sofa Seat
Refreshment Bar
Skylight
Cargo Hatch
Settee
Sofa Seat
Sofa Bed
Sofa Seat
Skylight
Sofa Bed
Sofa Seat
Settee
AIR WELL
Sofa Bed
Stewards Store Room
Skylight
Sofa Bed
Settee
Sofa Seat
Sofa Bed
Cargo Hatch
Sofa Bed
Sofa Seat
Sofa Bed

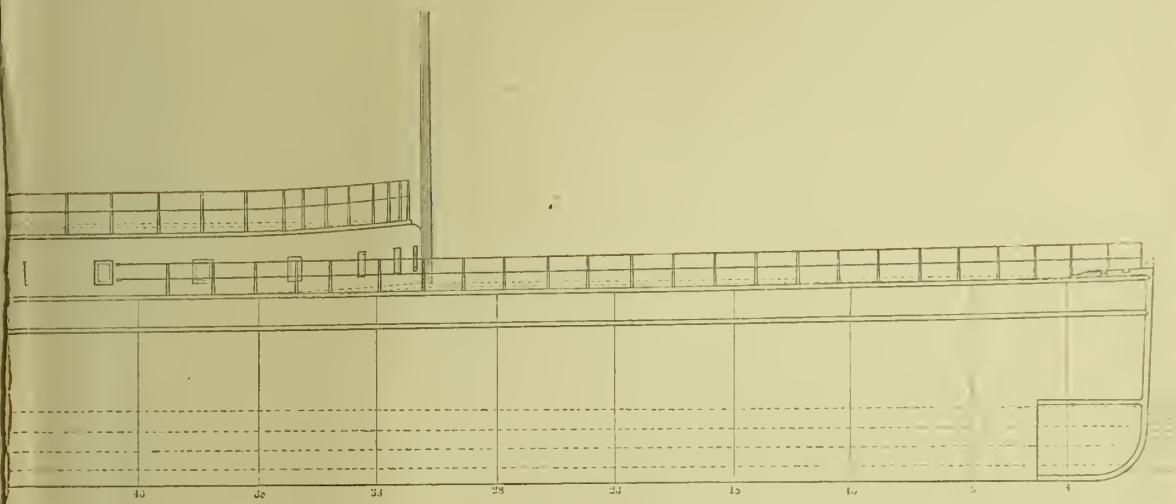
To illustrate Mr. G. C. Mackrow's Paper on Twin Ship Propulsion.

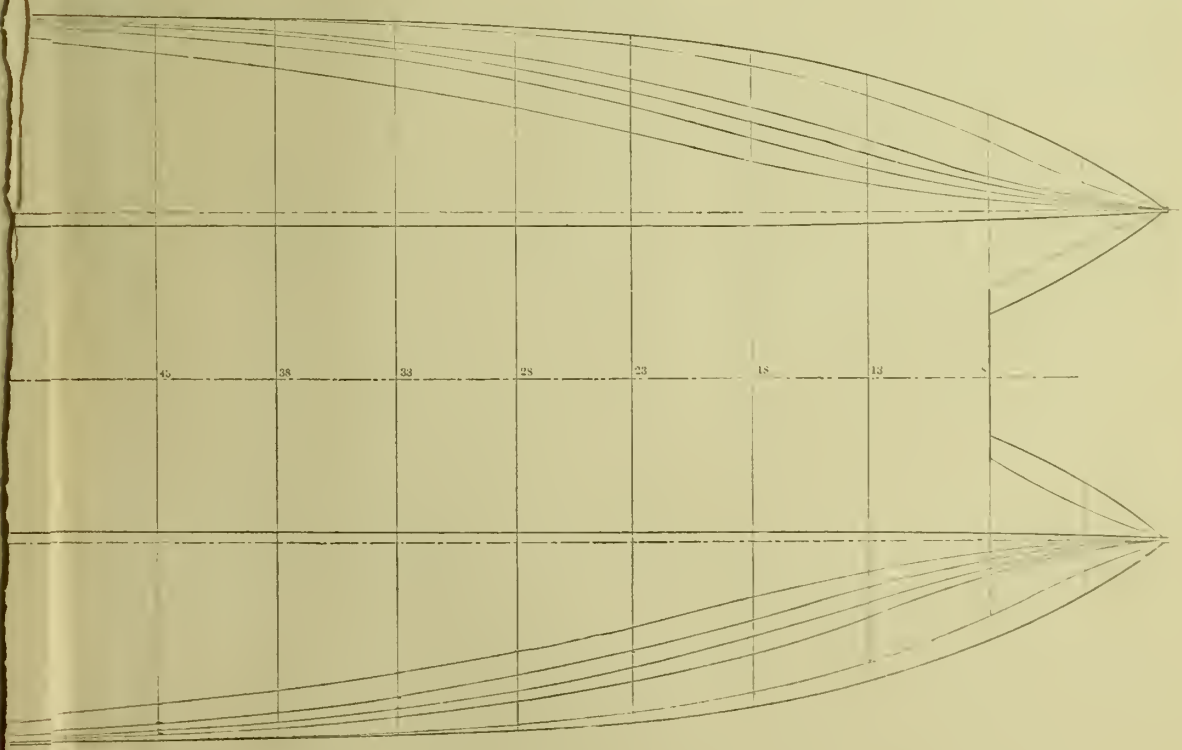
PROFILE.



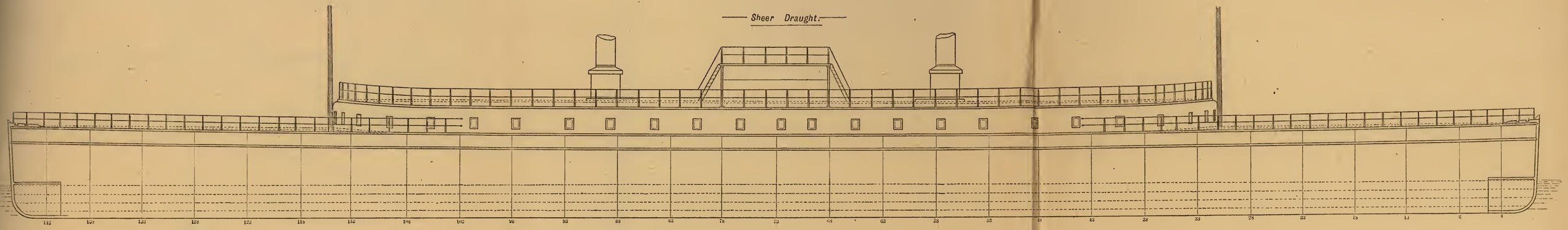
MAIN DECK PLAN.

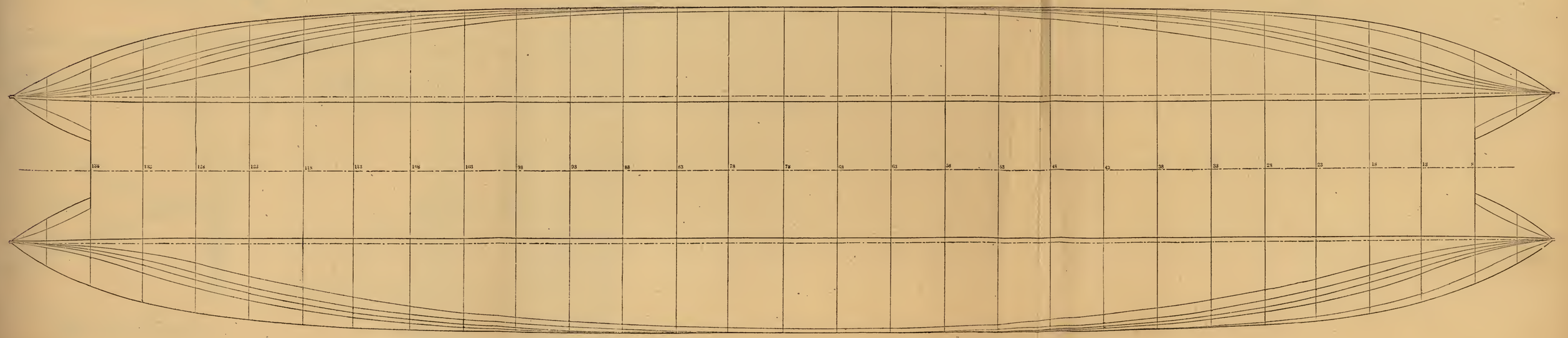



43
38
33
28
23
18
13
8
4

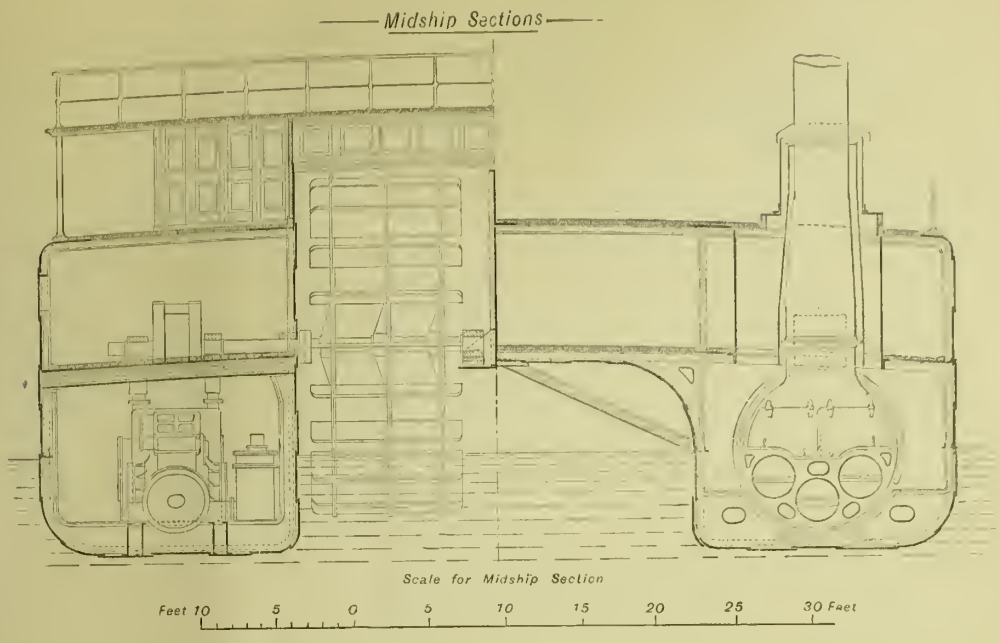

43
38
33
28
23
18
13
8

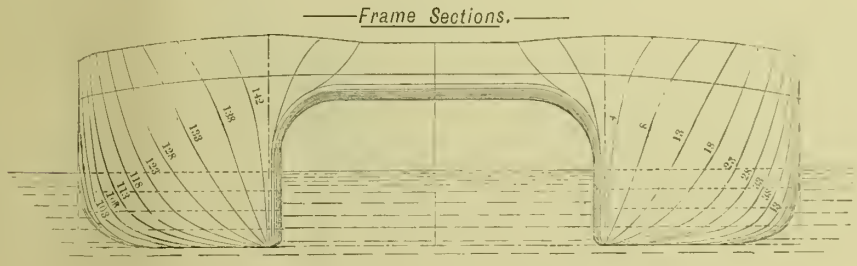
To illustrate Mr. G. C. Mackrow's Paper on Twin Ship Propulsion.

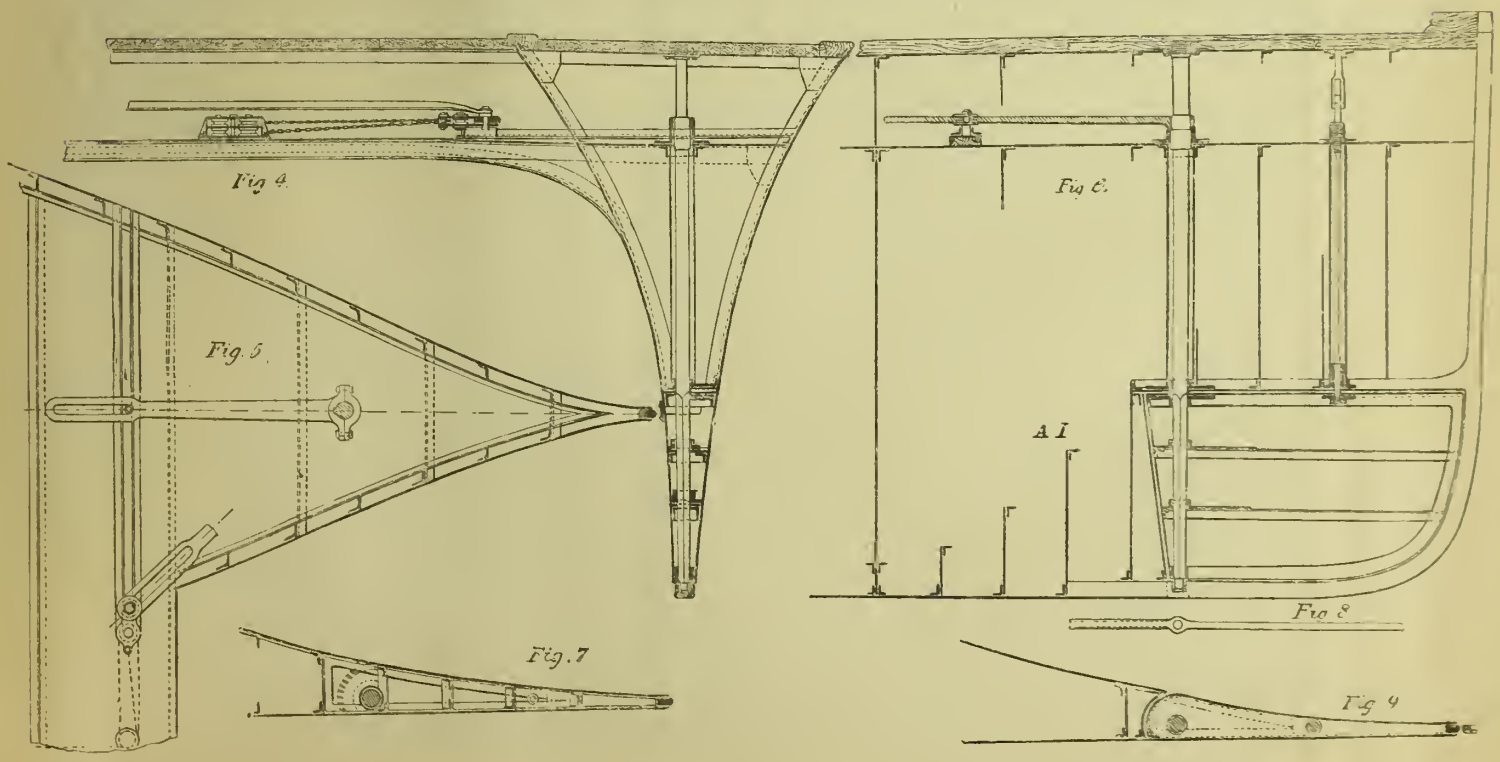




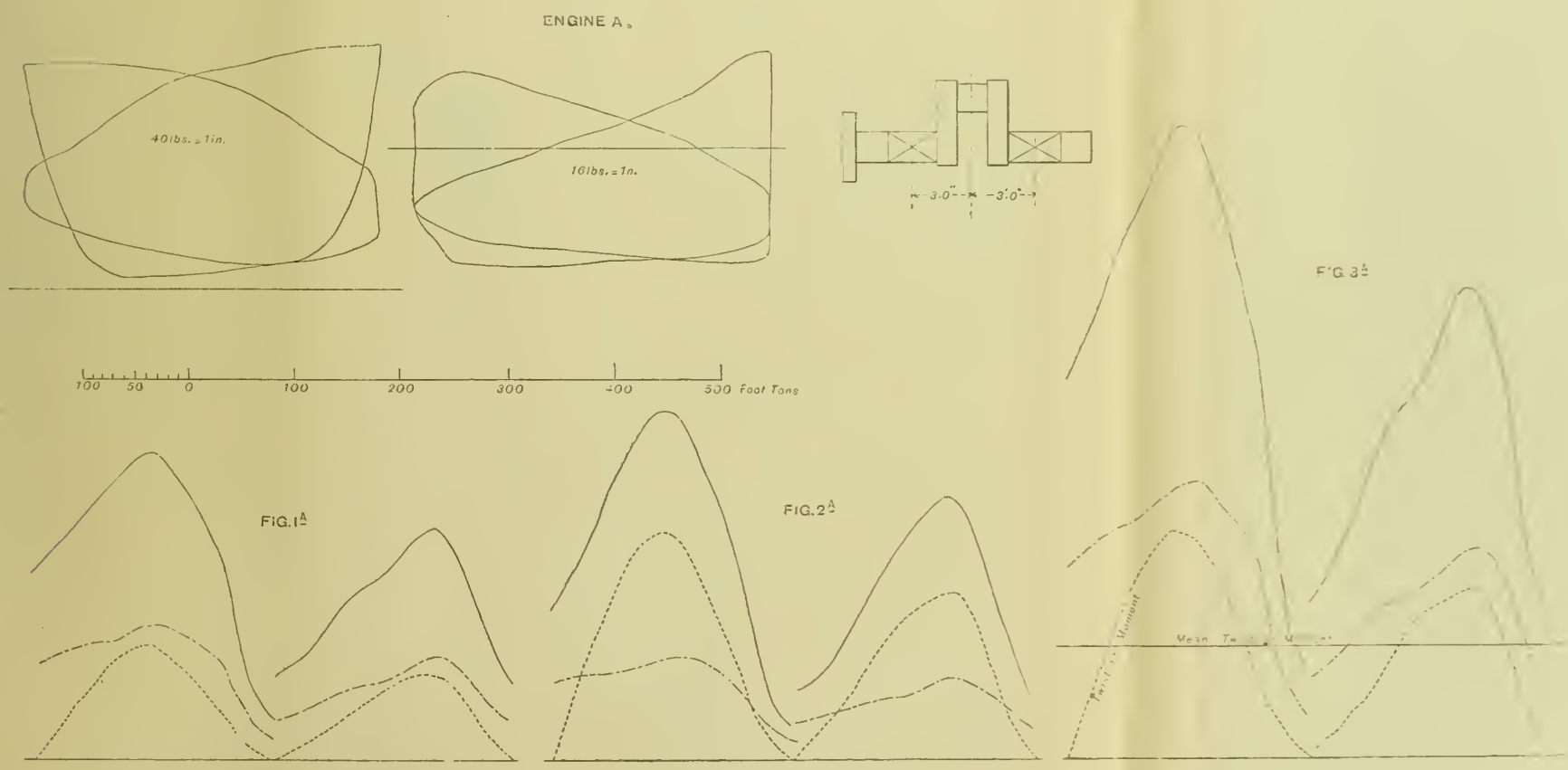
To illustrate Mr. G. C. Mackrow's Paper on Twin Ship Propulsion







To illustrate Mr. J. T. Milton's Paper on some of the Causes of Failure of Crank Shafts in Marine Engines.



Turine Engines.

FIG. B 1

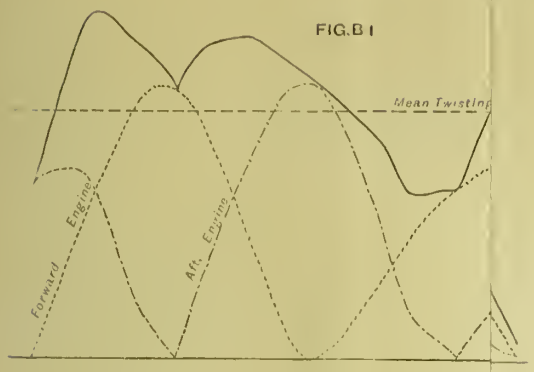


FIG. B 4

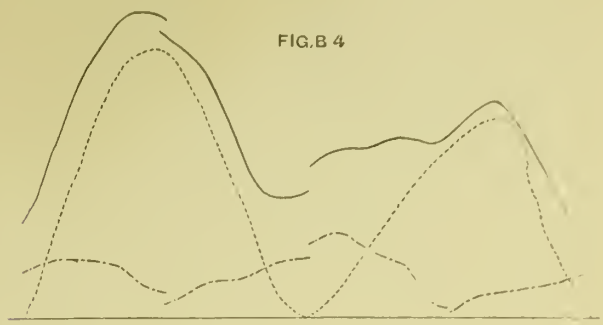


FIG. B 3

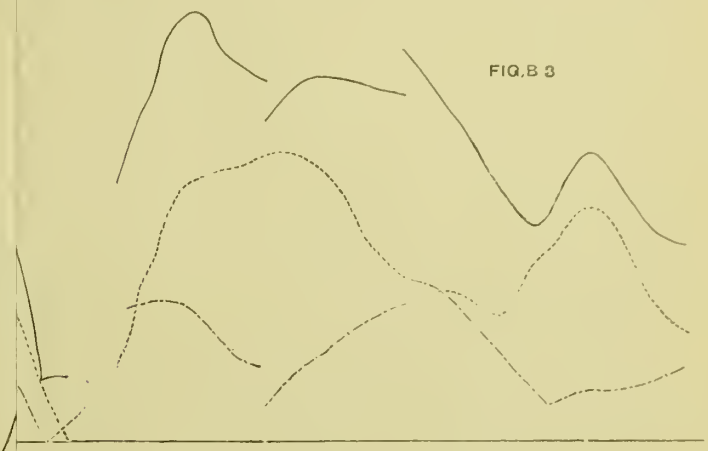
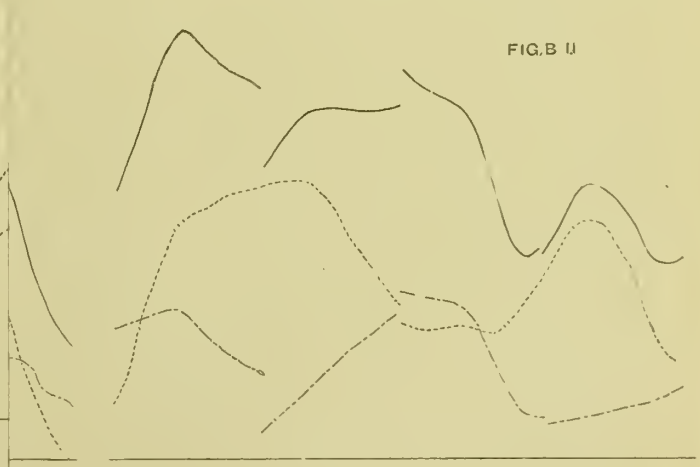
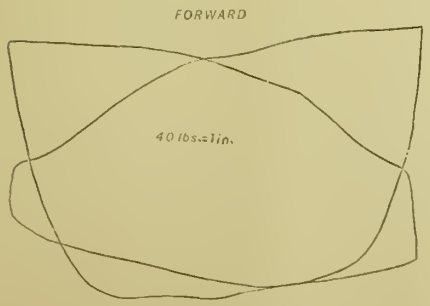
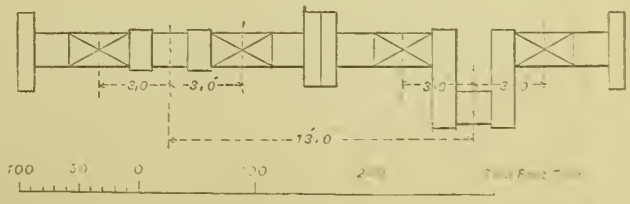


FIG. B 6

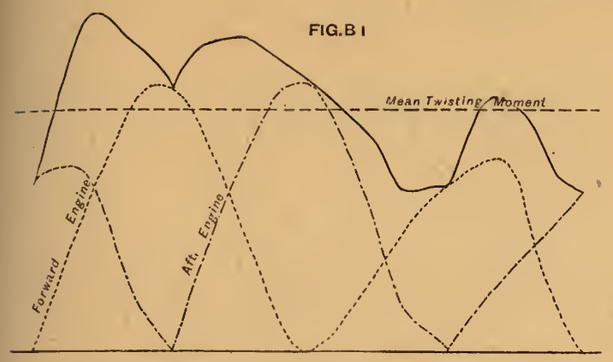
FIG. B 11

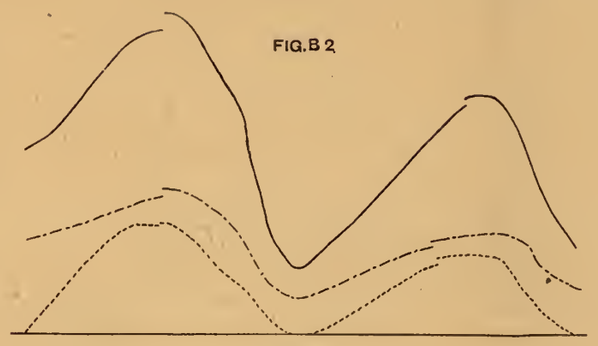


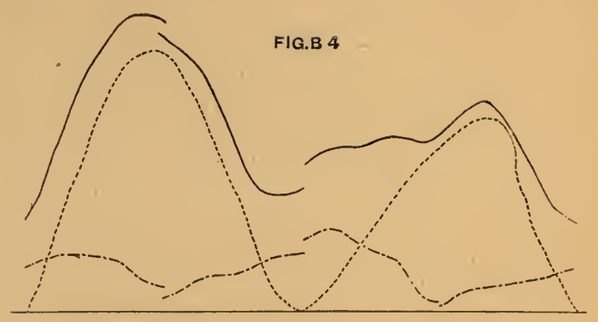


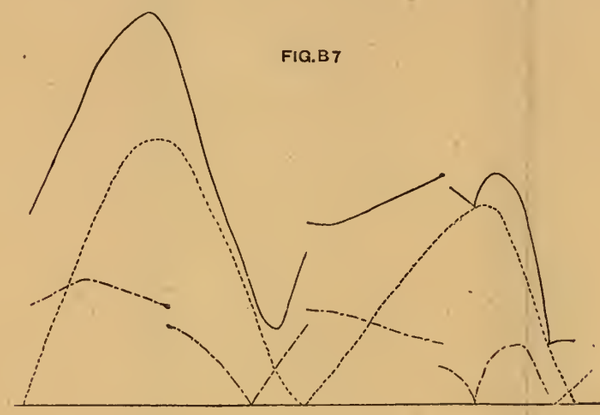


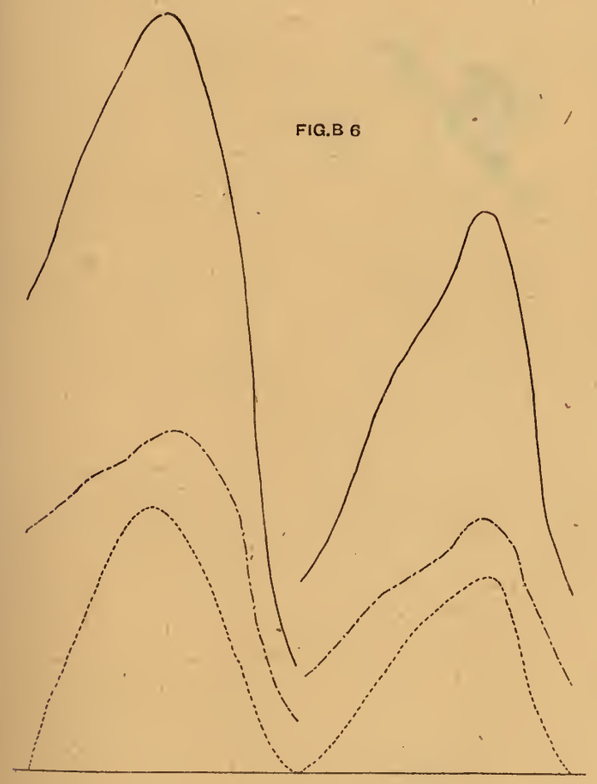
To illustrate Mr. J. T. Milton's Paper on some of the Causes of Failure of Crank Shafts in Marine Engines.

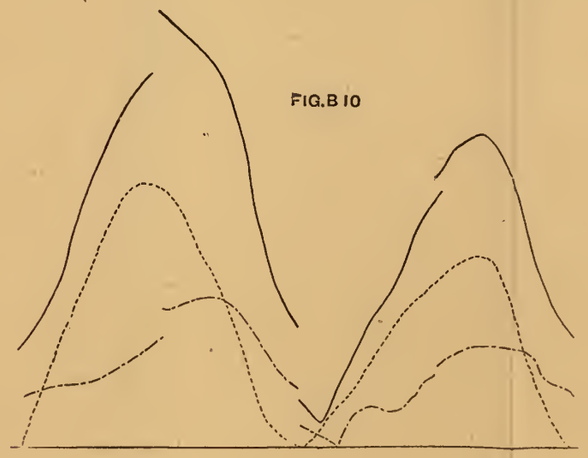


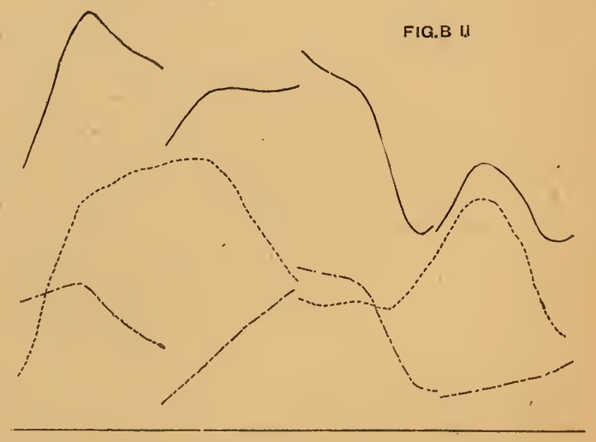


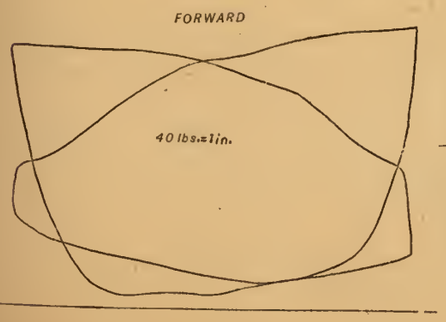


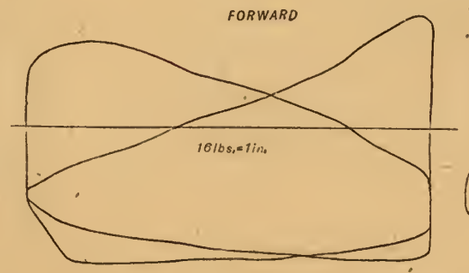


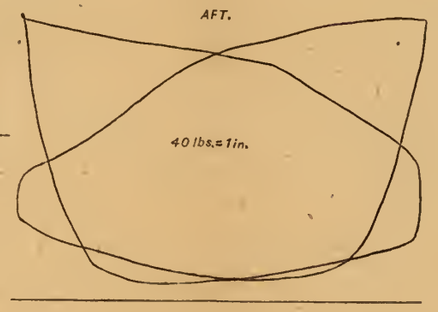


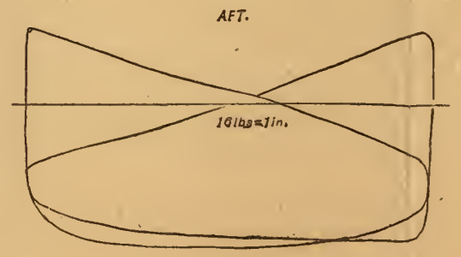


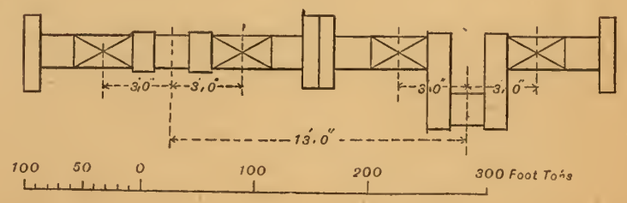




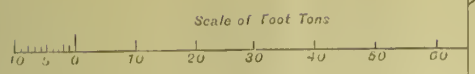








To Marine Engines.



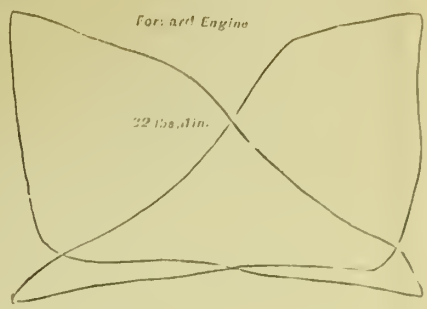


FIG. C 1

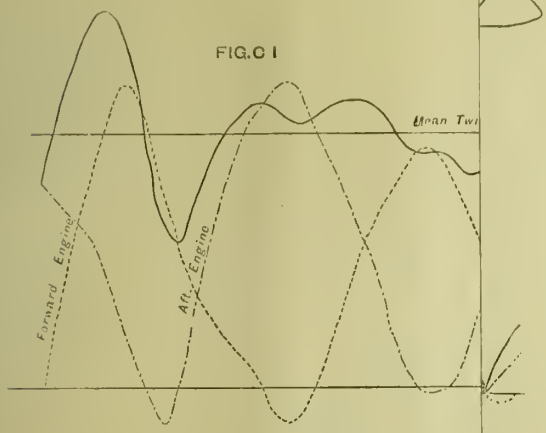


FIG. C 4.

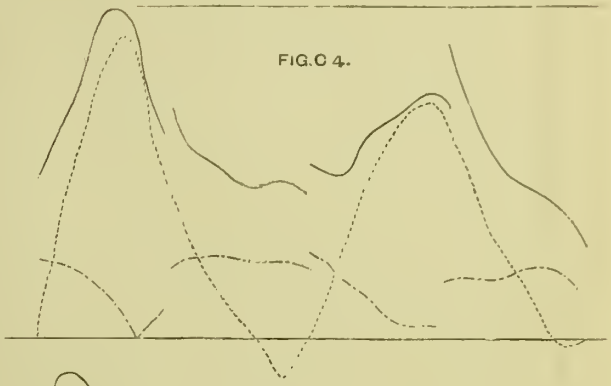


FIG. C 5

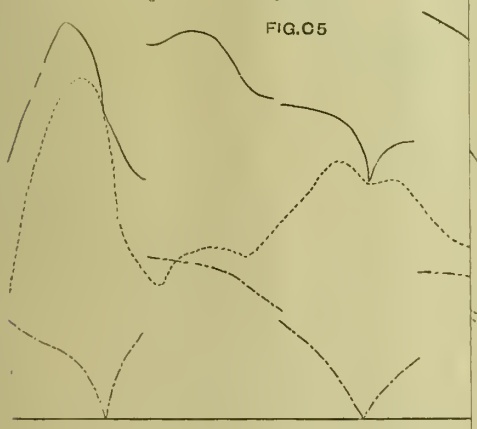


FIG. C 10

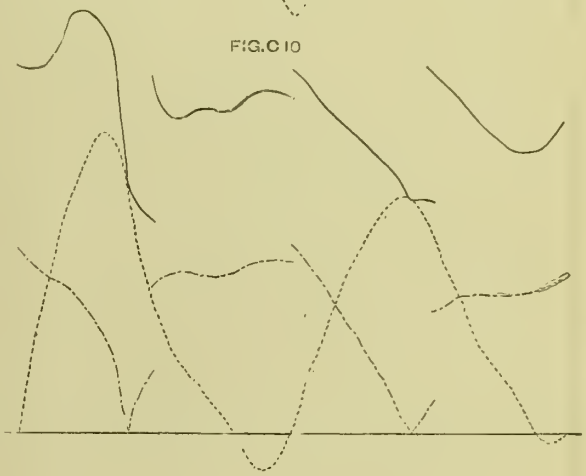


FIG. C 7

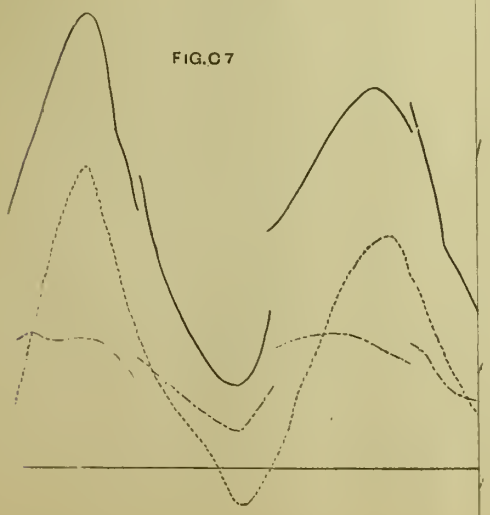
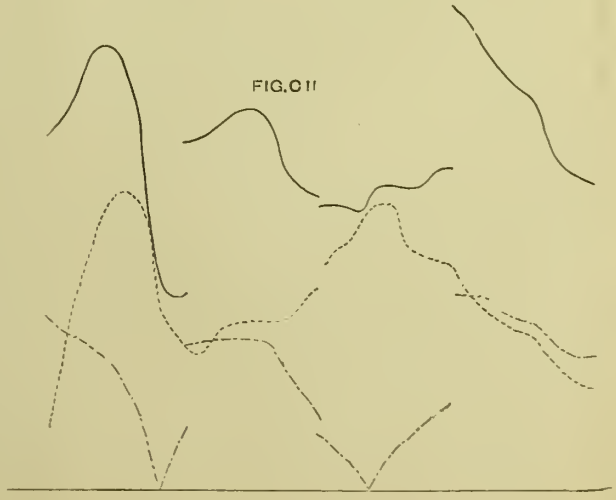
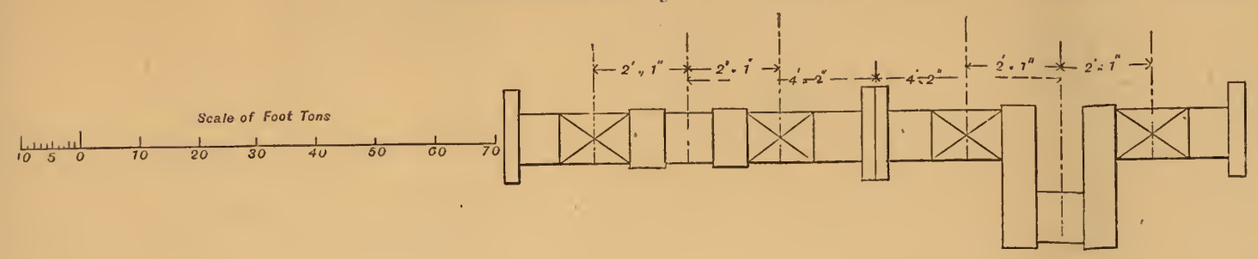
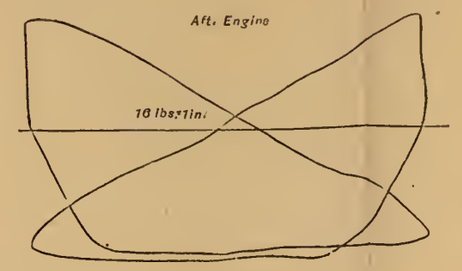


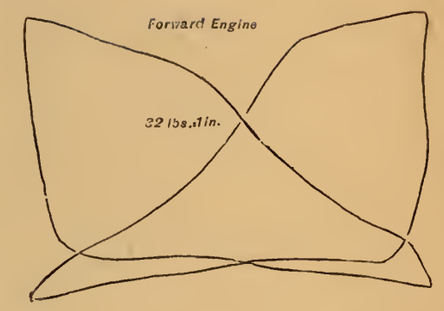
FIG. C 11

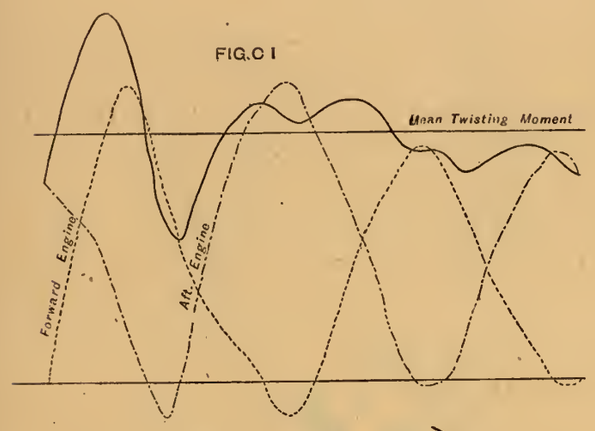


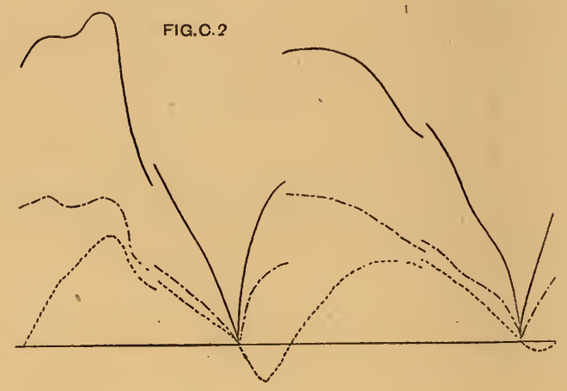
To illustrate Mr. J. T. Milton's Paper on some of the Causes of Failure of Crank Shafts in Marine Engines.

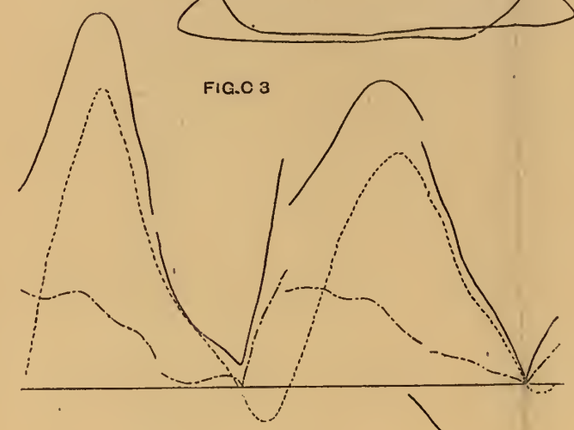


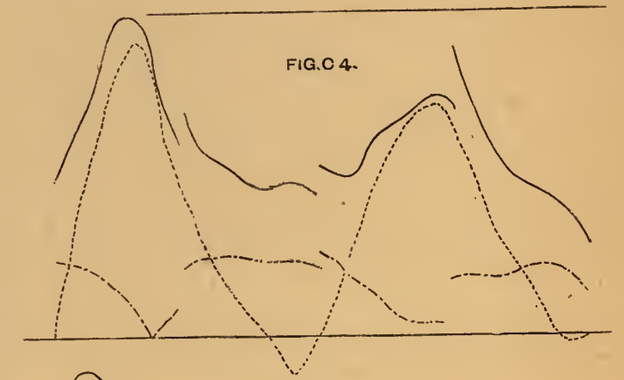


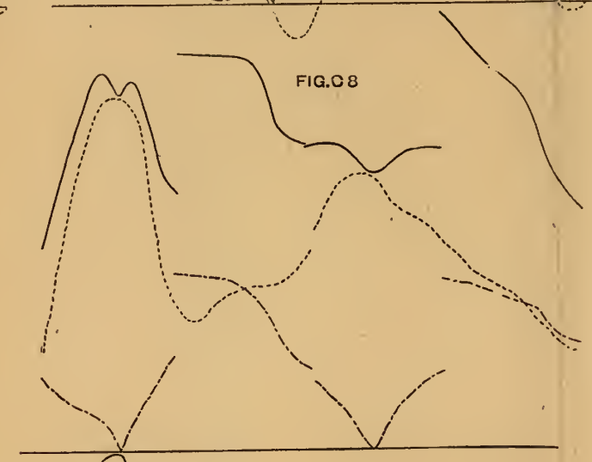


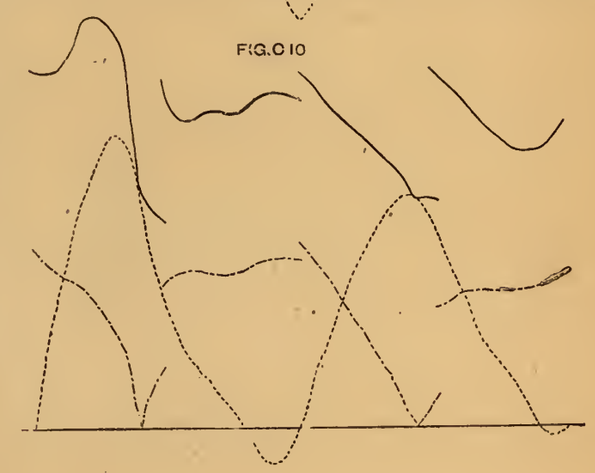


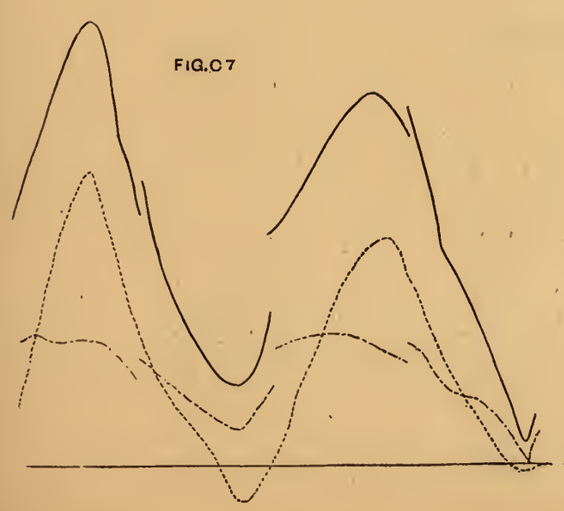


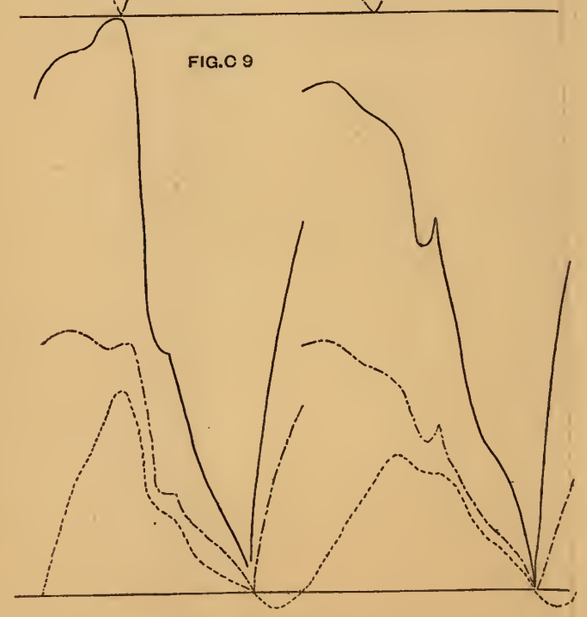


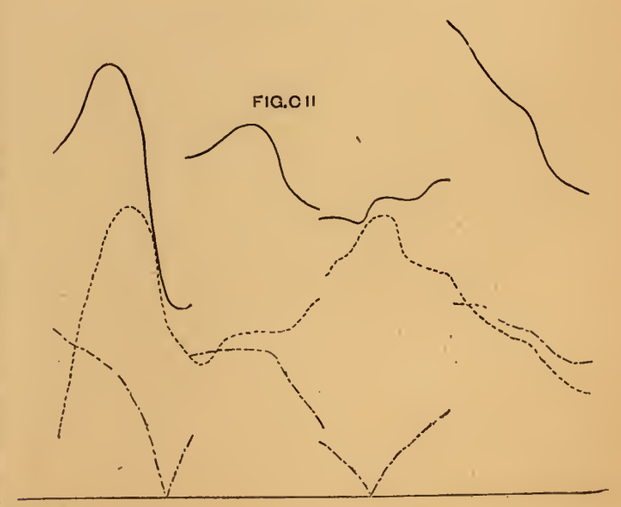












arine Engines.

FIG. D 1

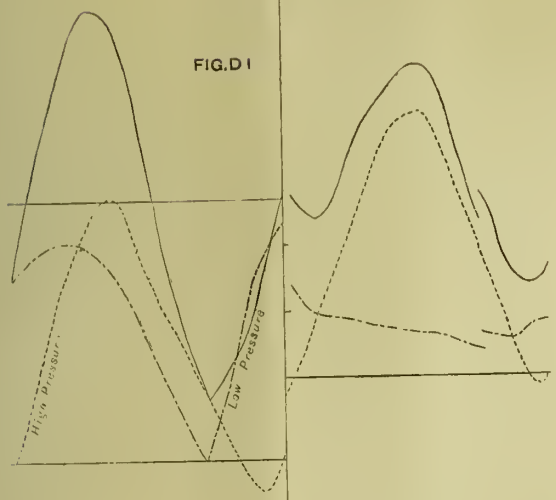


FIG. D 5

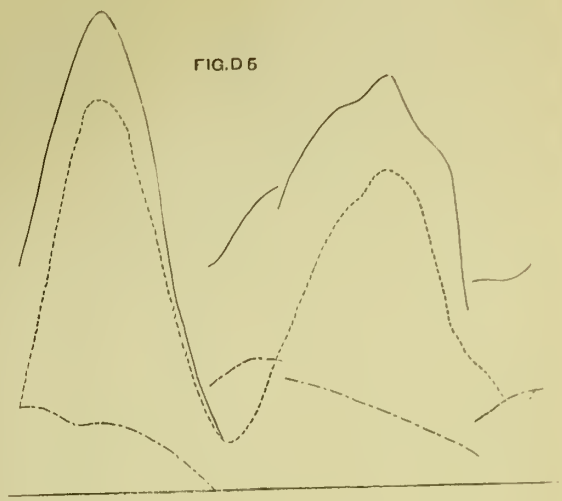


FIG. D 9

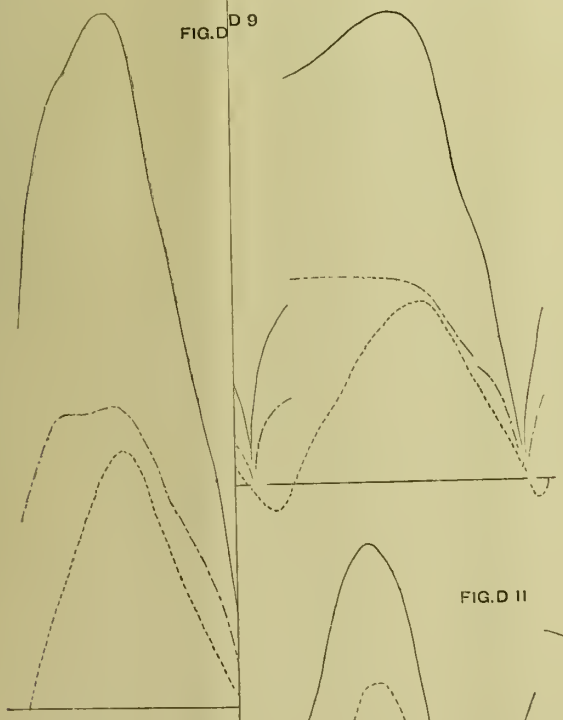


FIG. D 10

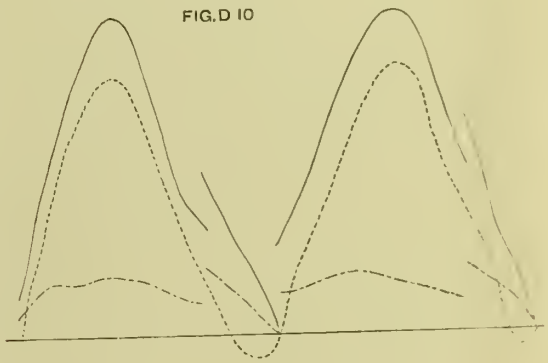
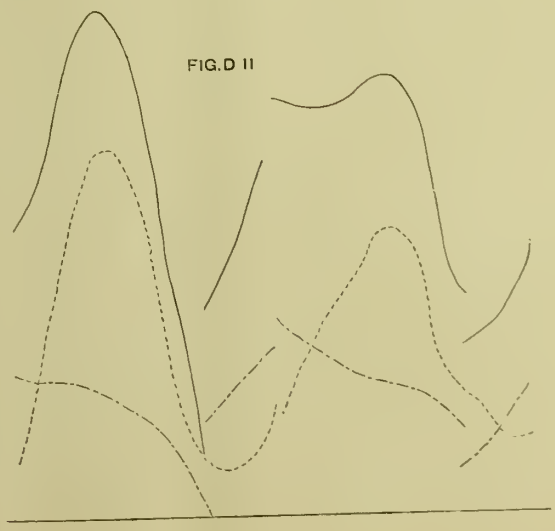
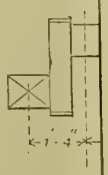
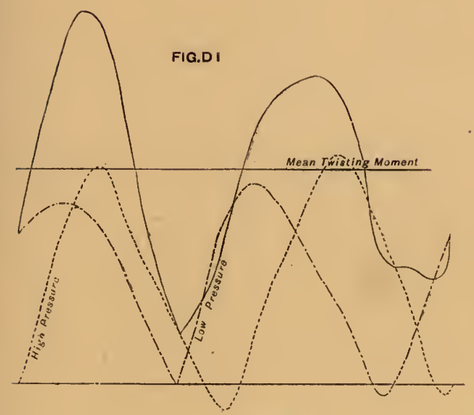


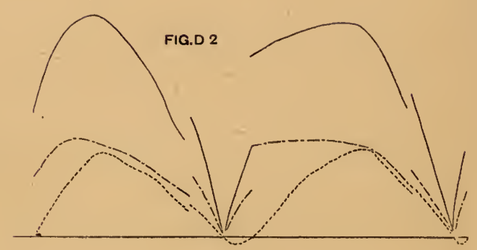
FIG. D 11

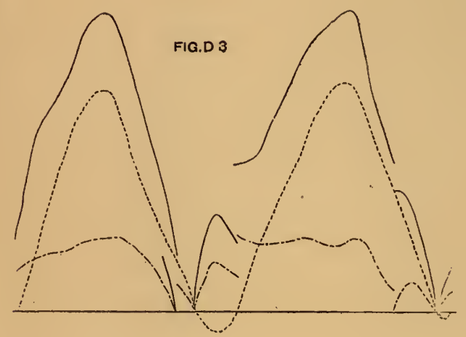


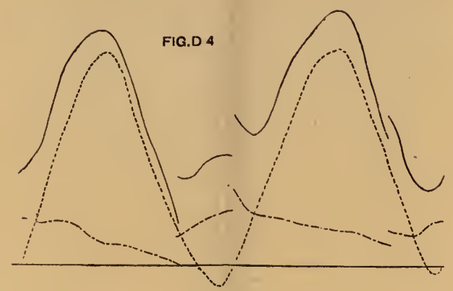


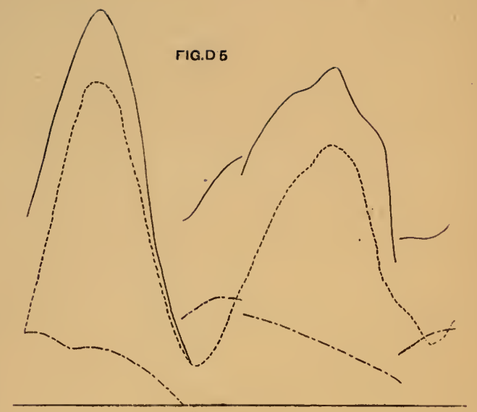
To illustrate Mr. J. T. Milton's Paper on some of the Causes of Failure of Crank Shafts in Marine Engines.

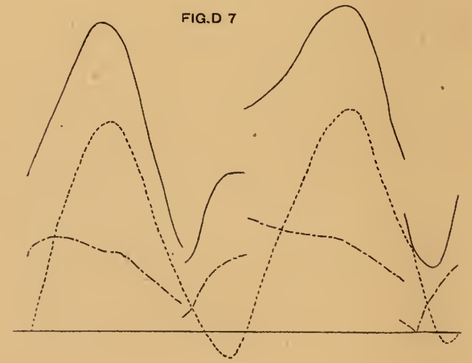


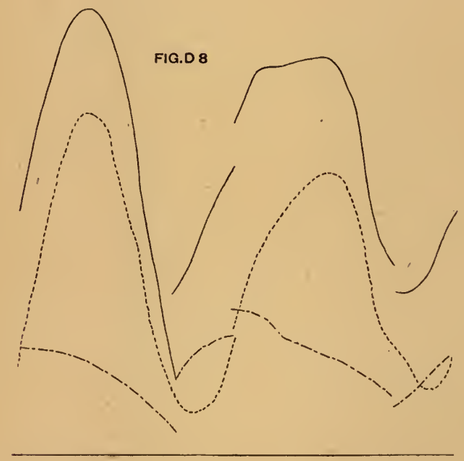


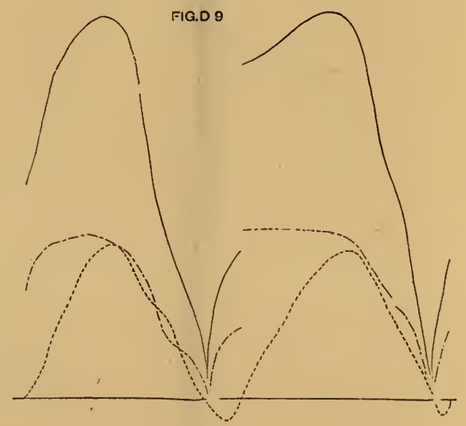


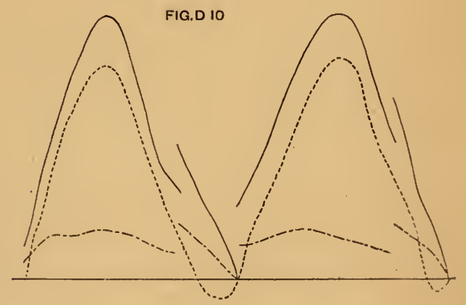


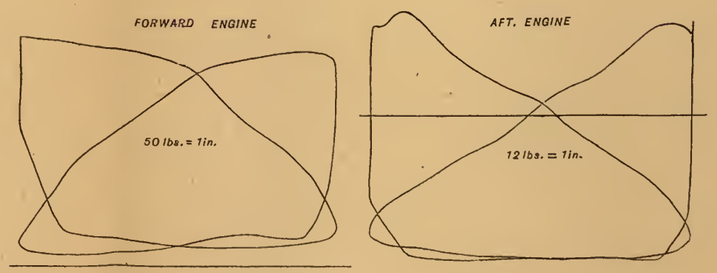


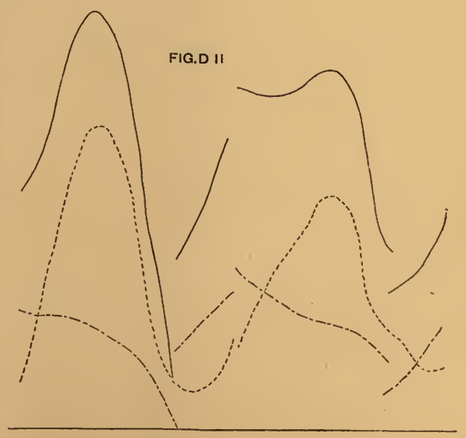


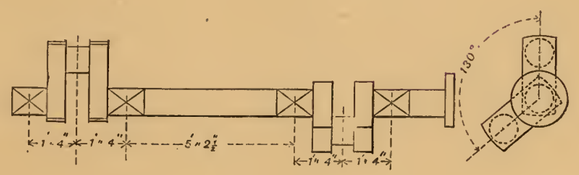


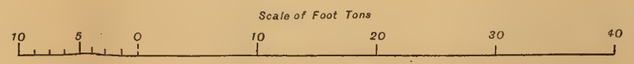




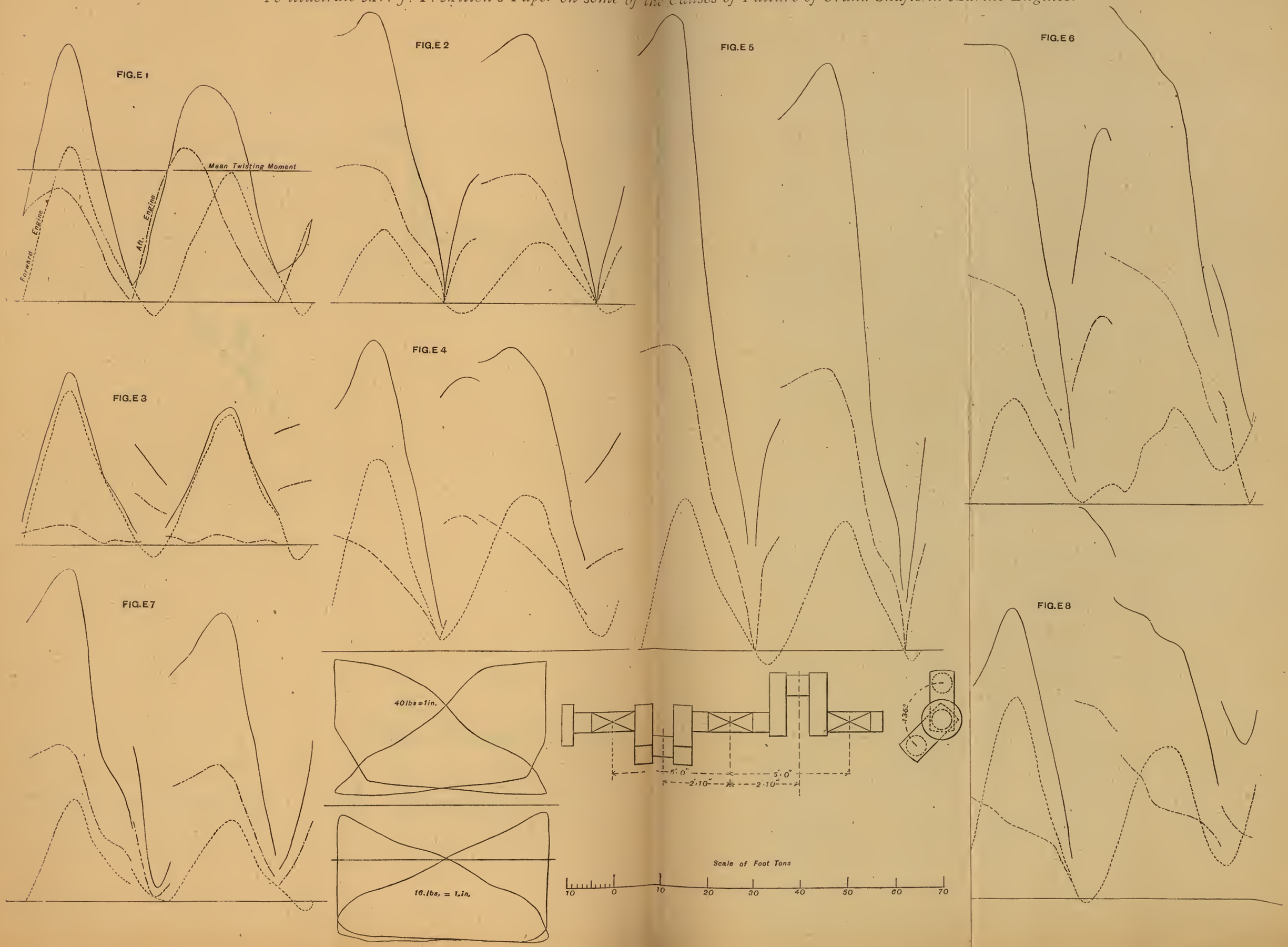




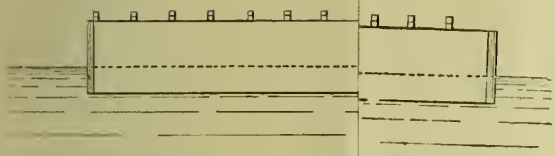


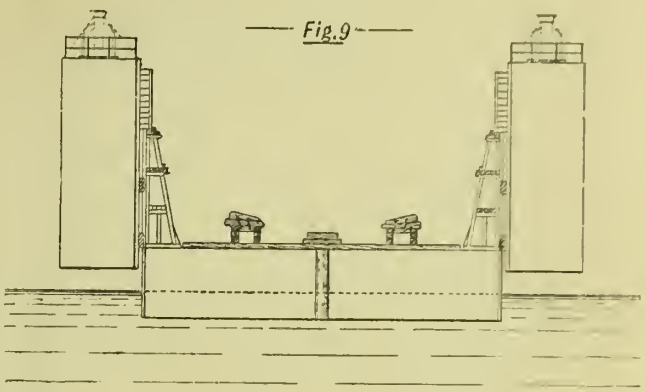


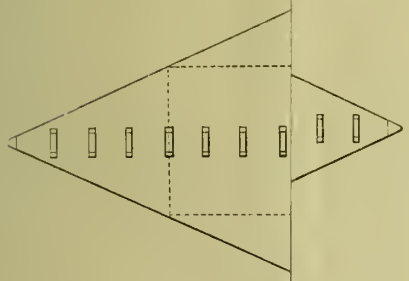
To illustrate Mr. J. T. Milton's Paper on some of the Causes of Failure of Crank Shafts in Marine Engines.

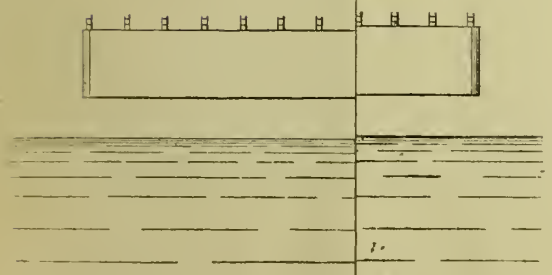


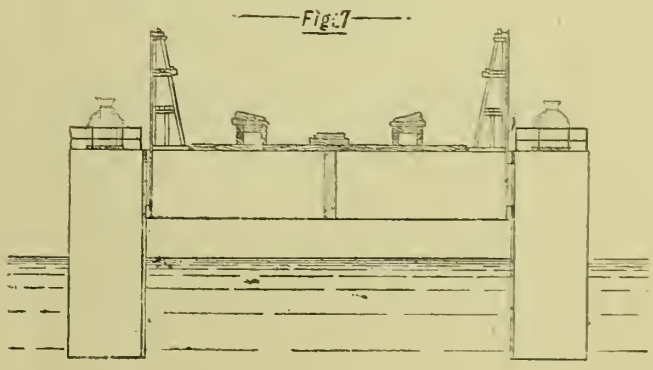
c Grid.



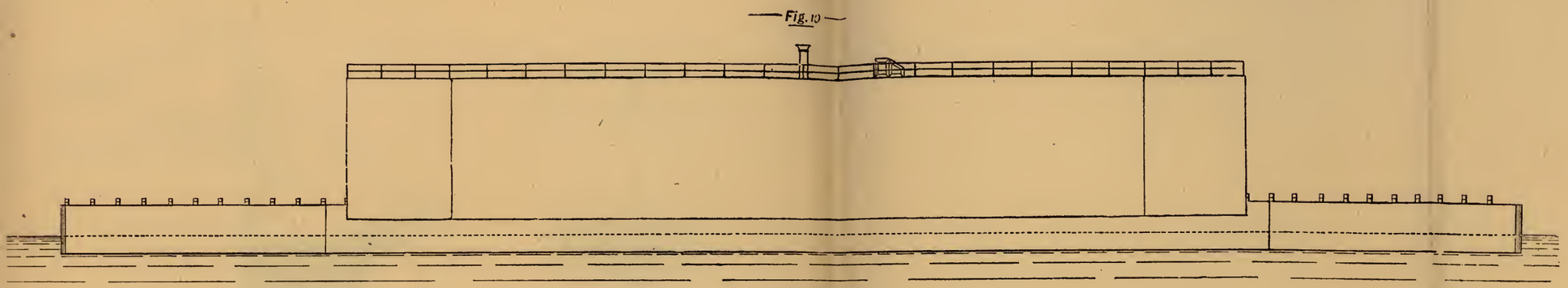


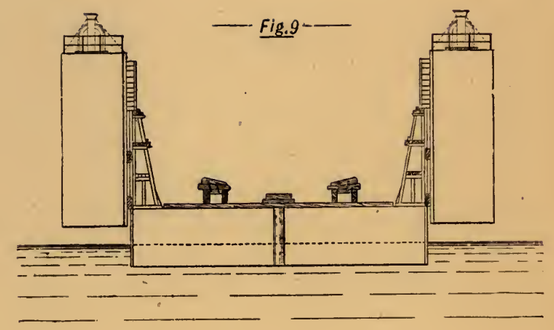


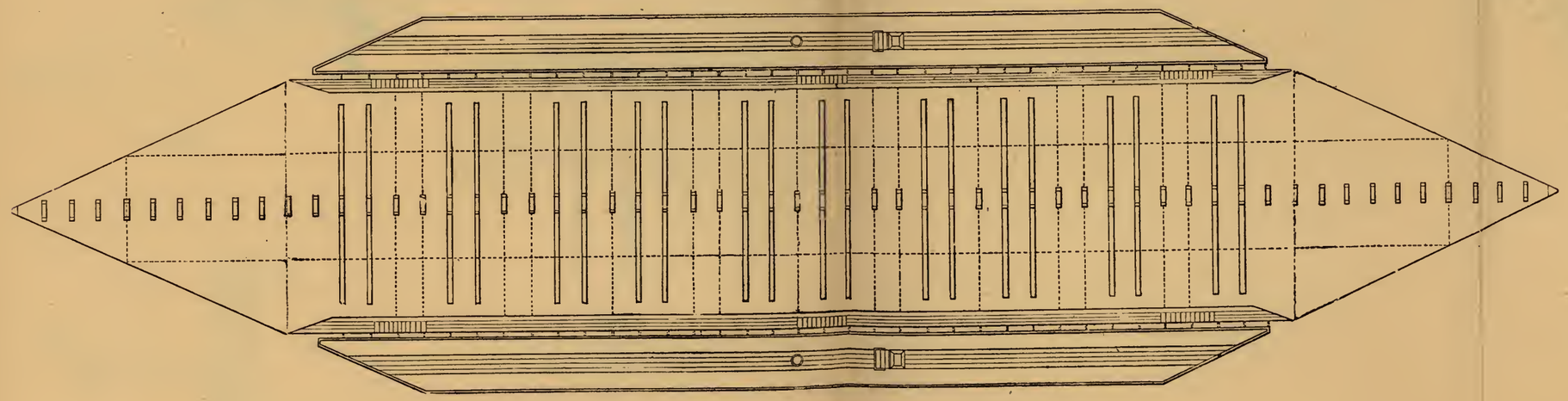


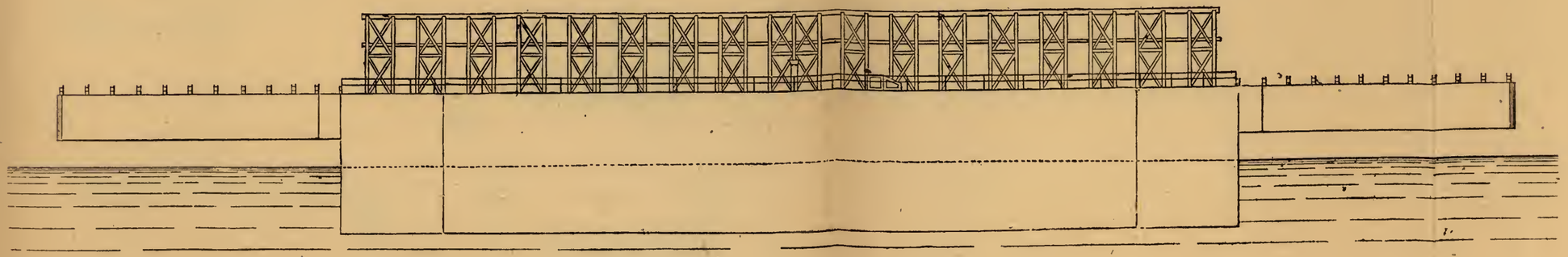


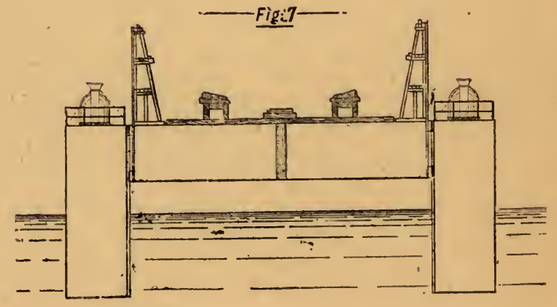
To illustrate Mr. Latimer Clark's Paper on the Double Power Floating Dock and Hydraulic Grid.



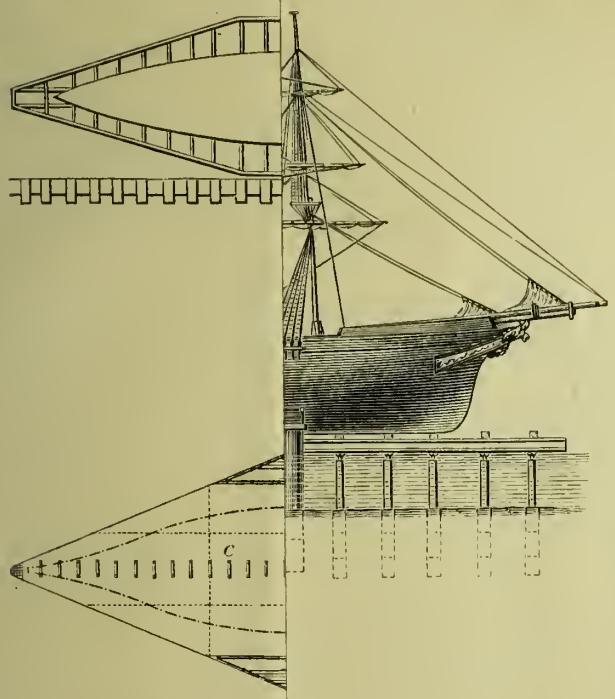








lic Grid.



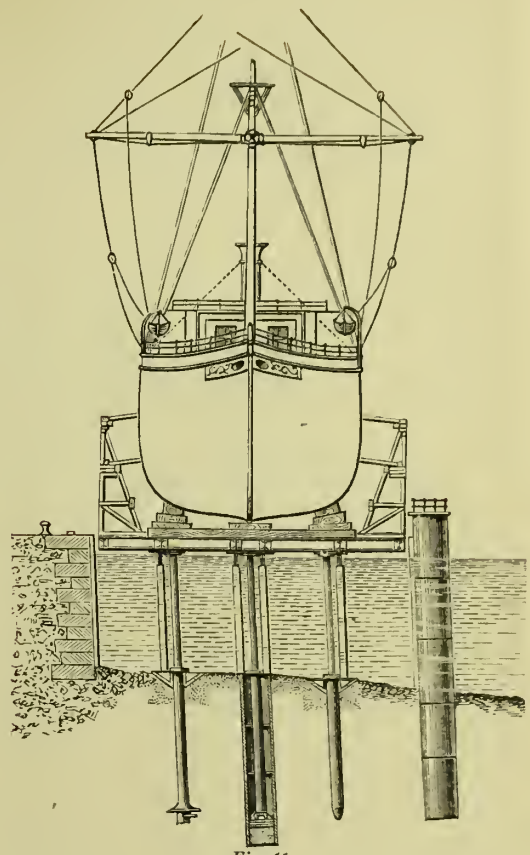
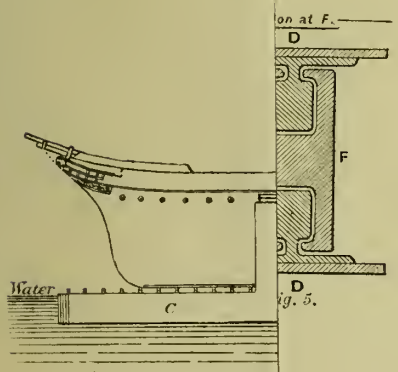
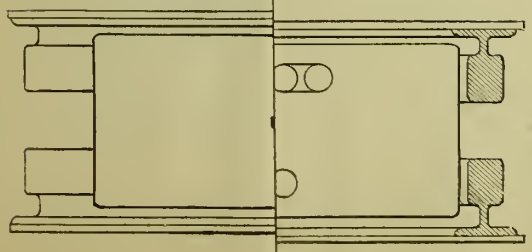


Fig. 11.



ig. 5.



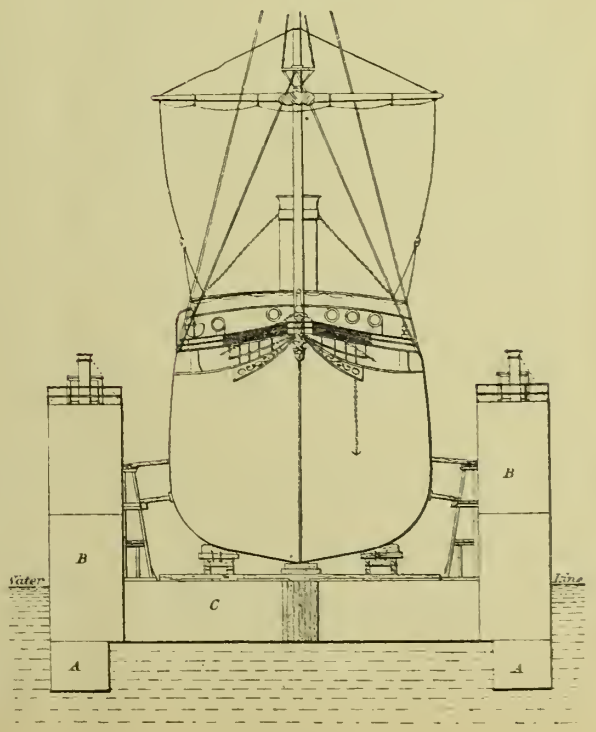


Fig. 2.

To illustrate Mr. Latimer Clark's Paper on the Double Power Floating Dock and Hydraulic Grid.

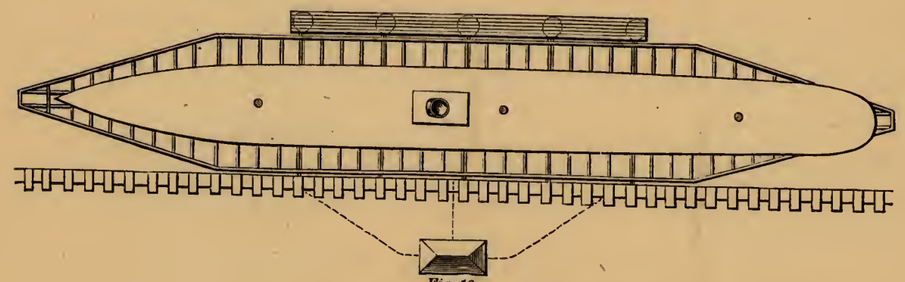


Fig. 13.

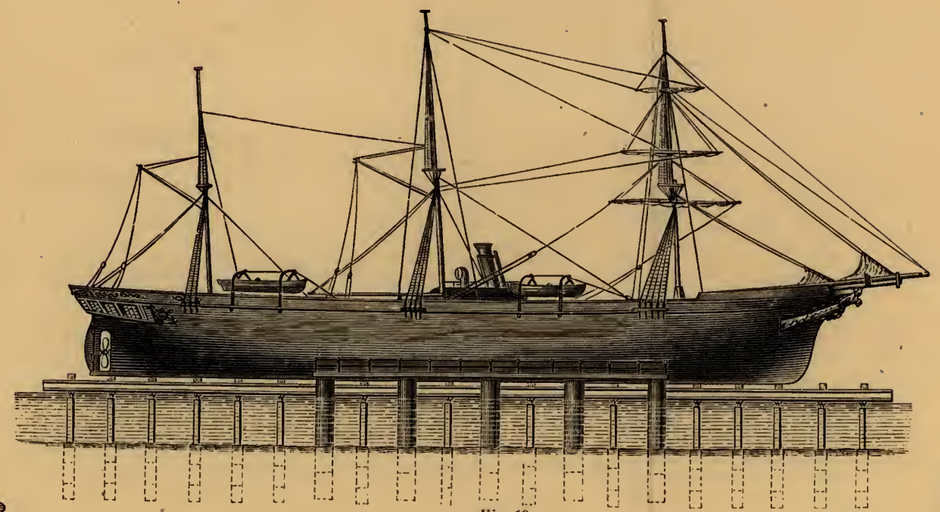


Fig. 12.

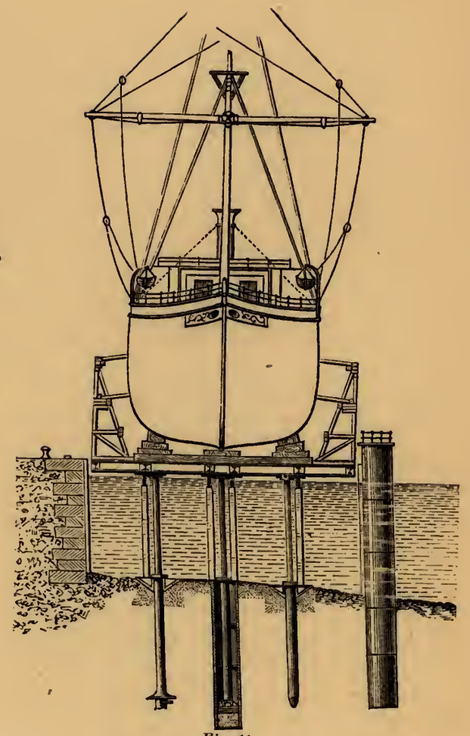


Fig. 11.

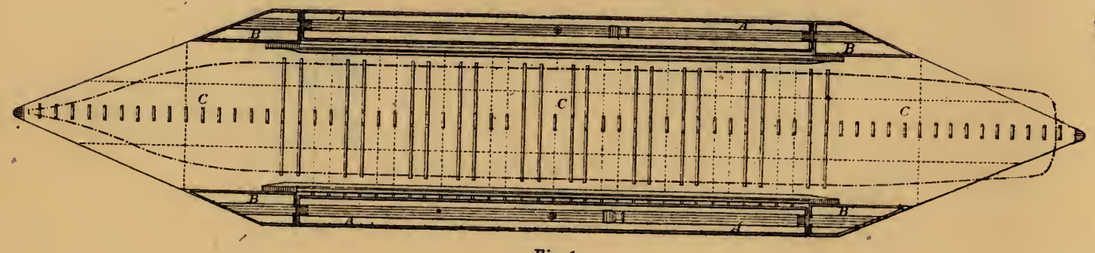


Fig. 1.

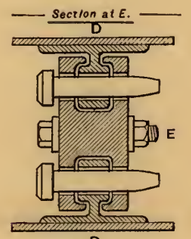


Fig. 5.

Sliding Side.

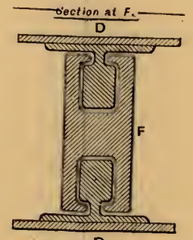


Fig. 5.

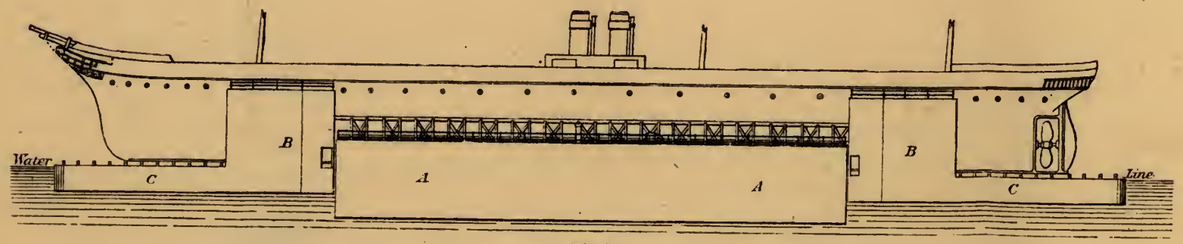


Fig. 3.

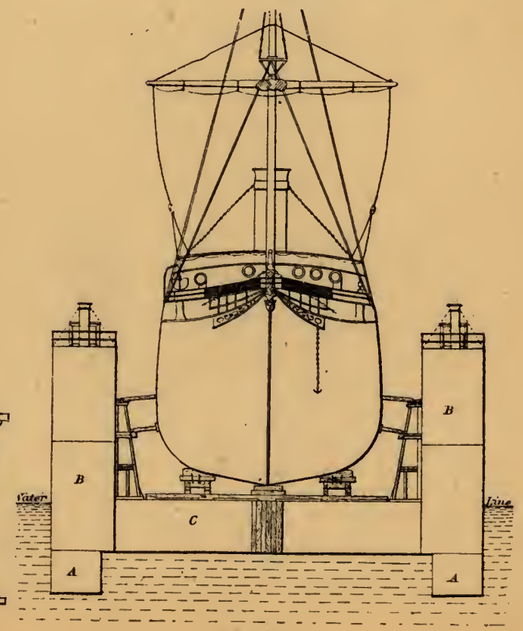
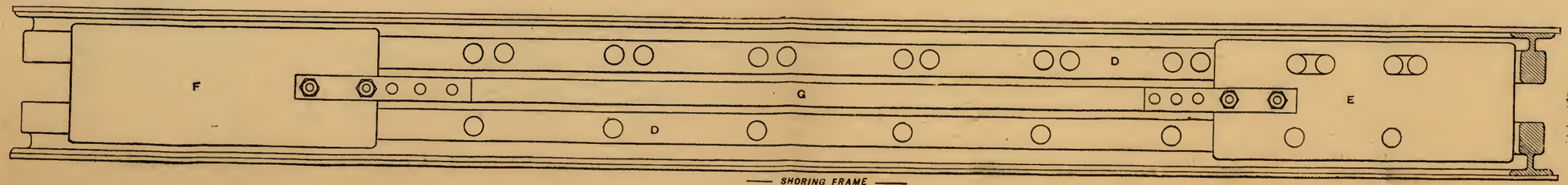


Fig. 2.

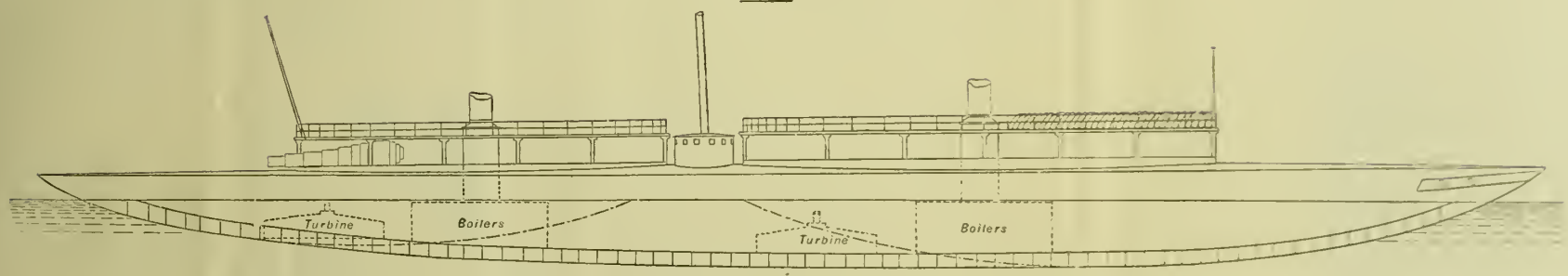


SHORING FRAME

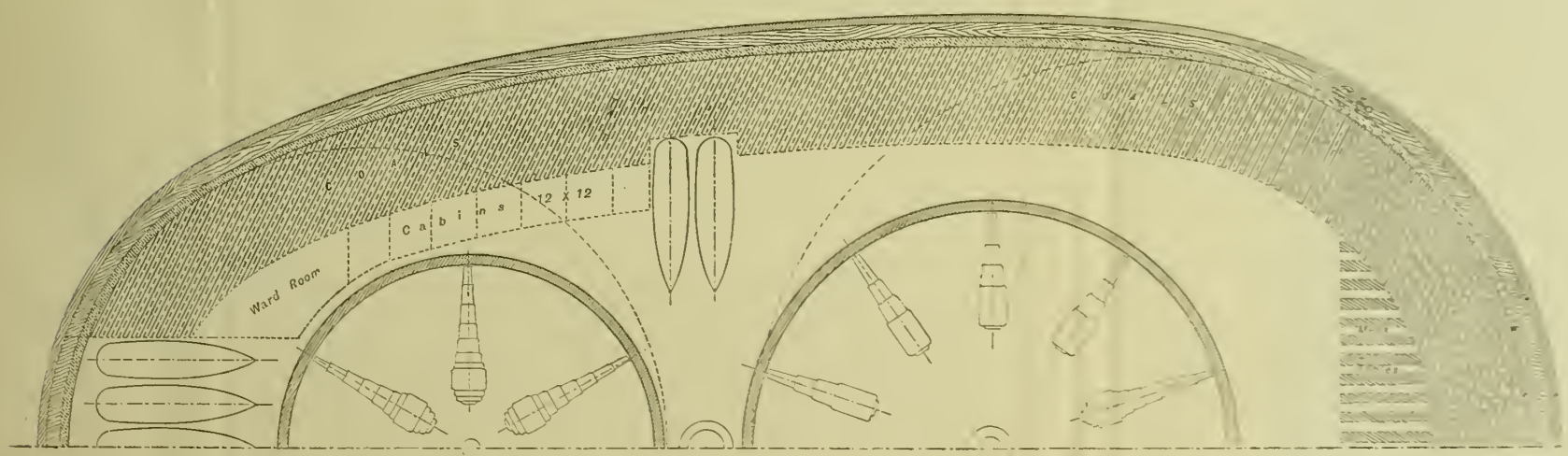
Fig. 4.

To illustrate Admiral J. H. Selwyn's Paper on the Most Powerful Ironclad.

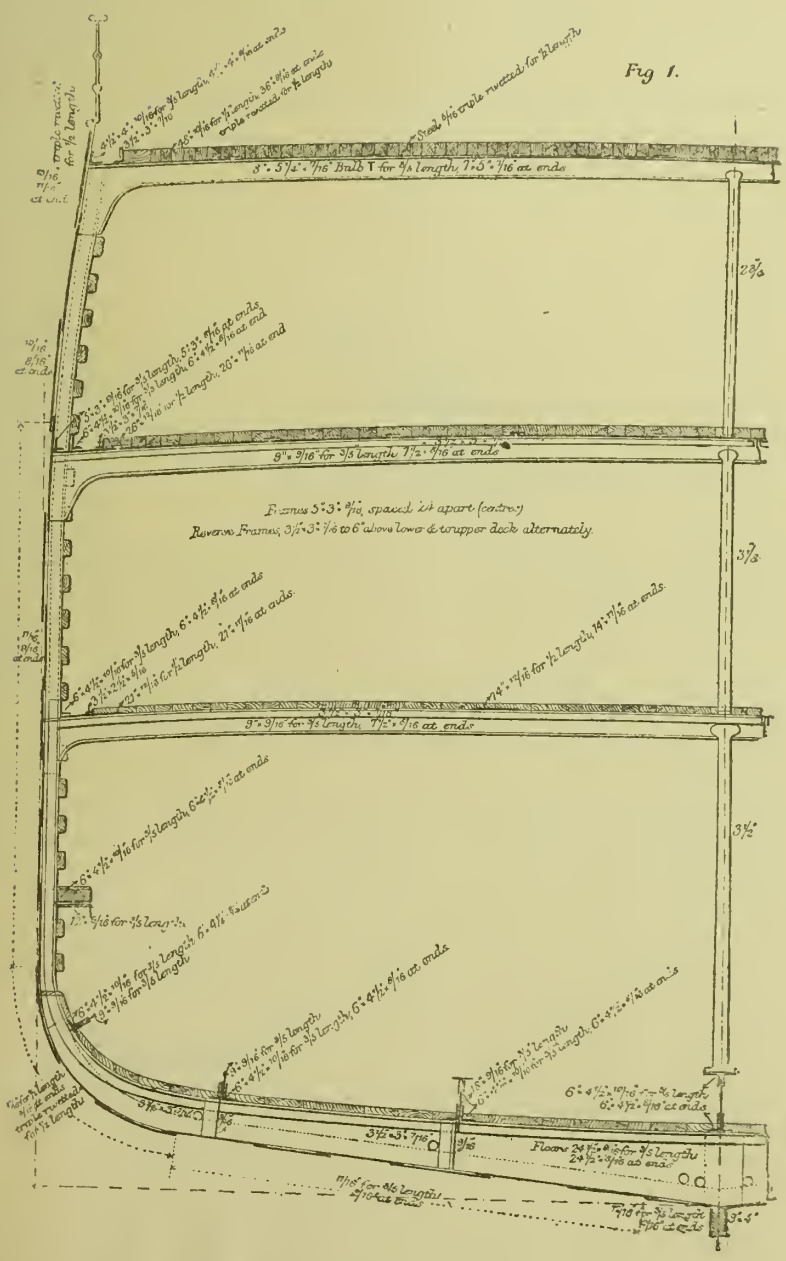
"THE BRITANNIA"
PROFILE.

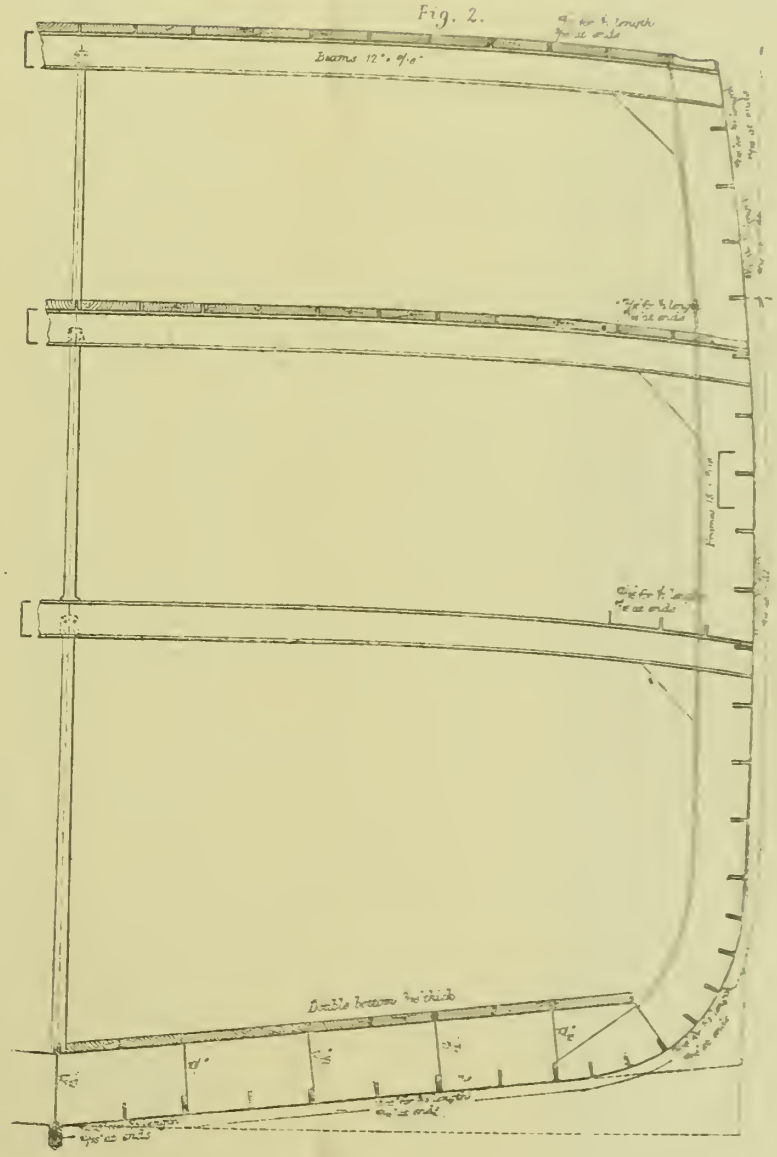


"THE BRITANNIA"
HALF BREADTH PLAN.



To illustrate Mr. J. Humphry's Paper on a System of Mechanical Shipbuilding





UNIVERSITY OF ILLINOIS-URBANA
3 0112 110331656